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JOURNAL,

CONTAINING THE

EARLIEST INFORMATION ON SUBJECT

OF

MEDICINE, SURGERY,

PHARMACY, CHEMISTRY,

AND

NATURAL HISTO

AND A

CRITICAL ANALYSIS OF ALL NEW BOOKS

IN THOSE

DEPARTMENTS OF LITERATURE.

CONDUCTED BY

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S. GOSNELL, Printer, Little Queen Street, Holborn.

To Dr. BATTY.

DEAR SIR,

AS you did me the favour to make a drawing of the ... of the following cafe, and was prefent at the operation, I ... queft you will infert the annexed account of it in your ve... ufeful Journal; and though it may not give rife to many new ideas on our mode of treating fuch cafes, it will fhew to what an uncommon extent fuch difeafes may arrive without being particularly detrimental to the conftitution, independent of the pain and diftrefs arifing from their weight. I am,

DEAR SIR,
Your obliged humble fervant,
RICHARD SIMMONS.

Cafe of an extraordinary Enlargement of the Clitoris. By RICHARD SIMMONS, *Member of the Royal College of Surgeons, Surgeon to the Britifh Lying-in Hofpital, Brownlow Street, &c. &c.*

[With a Plate.]

ON the 28th of Feb. 1800, Catharine Talbot, a healthy-looking woman, about thirty years of age, was placed under my care,* in the Parochial Infirmary of St. Martin in the Fields, where fhe had been admitted on account of her inability to follow her ufual occupation, which was that of working in the brick-fields, and other laborious employments, from a fwelling of great magnitude, as fhe defcribed it, hanging from her body; and which, upon examination, I found to be the *clitoris* enlarged to a moft enormous fize, gradually increafing in bulk from its ftem at the pubis. The circumference of the largeft part meafured fourteen inches, the circumference of the ftem five inches, and the length of the tumour nine inches. Its general appearance was fmooth and flefhy, and its furface covered with cuticle, and not redder than the ... general; round the bottom of the tumour, and all its ...

[illegible]ery unequal, being made up of a cluſter of ſwellings of a globular form, of different ſizes, from thoſe of large grapes to the ſmalleſt; the colour of theſe was redder, ſomewhat tranſparent and ſhining, but not inflamed or painful to the touch. When the tumour was held up, a detached lobe from the right ſide hung lower than the reſt, having the ſame globular appearances at its moſt depending part. The nymphæ and labia on both ſides, eſpecially near the perineum, appeared as if taking u[illegible] the ſame uncommon action with the clitoris, and felt [illegible]der; which might ariſe from the weight and preſſure of t[illegible]mour, as they were not much enlarged.

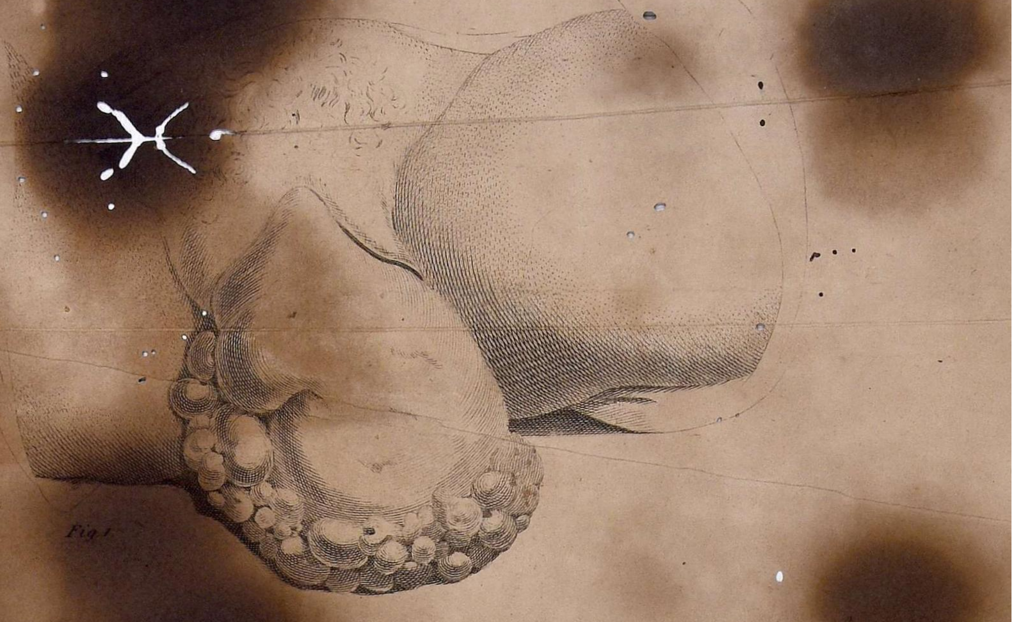
The moſt [illegible] circumſtance was, that her general heal[illegible] [illegible]as not at all [illegible]ed, her appetite was good, and ſhe men[illegible] [illegible]ed regularly; nor did this enormous maſs produce any [illegible] except from its weight, which gave her an uneaſy ſenſa[illegible] at the ſcrobiculus cordis, which was always relieved b[illegible] [illegible]pending the tumour, but which ſhe was either in general too careleſs to attend to, or the neceſſary ſupport too inconvenient to her to apply conſtantly.

The accouut ſhe gave of it was, that the part (clitoris) began to enlarge about four years ago, and without any apparent cauſe; and that it went on gradually increaſing in ſize for near three years, and that durir g the laſt year it had enlarged very rapidly. On queſtioning her very particularly, ſhe confeſſed having had a ſlight venereal complaint when ſhe was about twenty years of age, but has not the leaſt doubt of her having been perfectly cured of it, having ſuffered no interruption to her health in any way till the commencement of the ſwelling.

It will readily be granted that this was no common caſe; its extraordinary ſize and ſingular appearance neceſſarily rendered it an object of curioſity, and I occaſionally took ſeveral of my medical friends to examine it; many of whom had ſeen inſtances of enlarged and diſeaſed nymphæ and clitoris, but never any thing like the preſent. I ſaw no chance of relieving the poor woman without an operation; and ſhe being alſo well convinced of her conſtantly remaining a burden to herſelf and the world without its extirpation, readily agreed to any plan I might propoſe.

I determined therefore on uſing the knife, and requeſted Dr. Batty, Mr. Ford, Mr. Blair, and Mr. Morris, to favour me with their attendance on the 26th of March, when the operation was performed in the following manner:—A circular inciſion was made round the baſe of the ſwelling near the pubis, beginning at the inferior part on the right ſide, and ending on the [illegible]poſite ſide, and afterwards continuing the diſſection till [illegible] was removed. It was neceſſary to take up one veſſel

Fig. 1.



An extraordinary enlargement of the Clitoris.

Fig. 2.

Drawn by Dr. Batty. *Engraved by W. Skelton.*

Printed for R. Phillips, No. 71 St. Paul's Church Yard.

sel on each side; the three vessels of the clitoris were not materially enlarged, and were included in one ligature. The parts were dressed superficially, and thirty drops of laudanum given her. I saw her again in the evening, and the laudanum was repeated; she slept pretty well during the night, and was tolerably free from pain. On the 27th, some pain, tension and inflammation of the labia, came on, accompanied with fever; she took the saline mixture during the day, and at night the opiate was repeated. She discharged her urine freely. On the 28th, the tension and pain were increased; and not having had a stool since the operation, she took a laxative medicine, which had effect. On the 29th, the dressings were removed, having been loosened by a beginning suppuration; she passed the day rather better. On the 30th, the discharge from the sore was considerably increased, and the pain and tension diminished; one of the ligatures came away. From this time the sore continued to discharge freely for a few days, when both the other ligatures came off, the patient feeling extremely happy at the thoughts of having got rid of a disagreeable load, and with the prospect of the sore speedily healing; nor were her expectations disappointed, it gradually lessened in size, and, by the 21st of April, was completely healed, the labia, &c. assuming their natural appearance. She was discharged the Infirmary before the end of the month.

It must have occurred to those who have extensive practice in midwifery, to have observed children born with a peculiar conformation of the clitoris; instances where it has appeared considerably elongated, and covered with a large præputium, have given rise to mistakes of the sex of an infant, or have rendered it a doubt to which the child belonged. Specimens of this sort are to be seen in all anatomical collections, as well as examples of extraordinary enlargements of it in adults. And histories of such cases are to be met with in Riolanus, Bartholine, Schurigius, and others, some of whom believe the part endowed with a similar power to the penis in men. It would be of importance to discover whether any artificial means, or excess of venery, were capable of increasing its size; in the case above related I could not discover that at any period of its enlargement it had the power of erection, or was converted to any improper purpose.

It appears that an enlarged clitoris is almost endemial in some countries, particularly Egypt and Darfur, where the excision of it is very commonly practised, and the operation is performed a little before the period of puberty, or at about the age of eight or nine years. This custom is mentioned by Strabo, and also by Albucasis, in his 7th chapter, who observes, that

that every parent knows when a child has these parts longer than ordinary, and cut and burn them off while girls are very young. De Graaf was also acquainted with this, and gives his approbation of the operation as highly necessary as well as decent: "Estque hujus partis chirurgia orientalibus tam necessaria quam decora." And Mr. Brown, our countryman, the celebrated traveller into Africa, tells us, that thirteen or fourteen young females underwent the operation in a house where he was. It was performed by a woman, and some of them complained much of the pain both at and after it. They were prevented from locomotion, but permitted to eat meat: the parts were washed every twelve hours with warm water, which profuse suppuration rendered necessary; at the end of eight days the greater part were in a condition to walk, and liberated from their confinement; three or four of them remained under restraint till the thirteenth day. The reflection that naturally arises from this fact is, that there is no hazard in performing the operation at a very early period; and the success attending the extirpation of the prodigious one in the case I have related, is a sufficient evidence of the safety of an operation at a more advanced stage of the disease.

EXPLANATION of the PLATE.

Fig. 1. The upper side of the tumour. Fig. 2. The under part.
The weight of the tumour after extirpation was twenty-eight ounces.

To Dr. BATTY.

Dear Sir,

I Conceive that your valuable Journal serves, among other useful purposes, as a Court of Honour, to which any Member of the Faculty may appeal, respecting the misconduct of another. Allow me, therefore, through this channel, to point out to the censure of all professional gentlemen, a very unusual species of attack, which has been made upon me in a late publication.

The book to which I allude, is an Essay on *Phlegmatia Dolens*, written by Dr. Hull, of this place; in the course of which he has undertaken to controvert my Essay *On a Disease of the Lymphatic Vessels, hitherto misunderstood*, which was inserted in the third volume of my Medical Histories and Reflections. The works of every author are open to criticism, and I have never betrayed impatience when objections have been fairly and candidly

candidly made againſt any part of mine. The public opinion concerning my writings is already formed; and it would have given me no concern, that they were diſapproved by pedants without learning, and compilers without diſcernment. But the offence which I mean to denounce is of a moſt ſerious nature.

In my Eſſay, which was publiſhed in 1798, I had introduced an account of the caſe of Jane Waters, a home-patient of the Mancheſter Infirmary, who had been recently under my care, and I mentioned that it was an accurate ſtatement. I have lately ſeen with aſtoniſhment, in the Eſſay on *Phlegmatia Dolens*, that Dr. Hull, who has no connection with the Infirmary, or intercourſe with me, *has called upon this woman*, for the purpoſe of correcting my account of her caſe! And that, from the confuſed recollection of this poor ignorant creature, at the diſtance of *nearly two years* from the occurrence of the diſeaſe, he has had the preſumption to publiſh a ſtatement of her complaint, contradicting mine in ſeveral important particulars! With a man who ſhews a total diſregard of profeſſional character, by the avowal of ſuch conduct, I cannot ſtoop to a controverſy.—Give me leave to addreſs ſome ſhort obſervations to your readers.

My reports of this caſe, as of many others, were drawn from notes which I took at every viſit I paid to the patient.—They ſhew the regular progreſs to a cure of the lymphatic inflammation, from the 3d to the 9th of January, 1798; and of an inflammatory affection of the knee, which was excited by her imprudent exertions about the 16th of that month. My report on the 21ſt of January, runs thus: "The affection of the limb is completely removed. She has no complaint, excepting debility, and a cough, which ſhe had contracted previouſly to her lying-in." On referring to the liſt of home-patients,* I find that Jane Waters was made an out-patient on the 27th of the ſame month.† In oppoſition to theſe facts, Dr. Hull has produced a narrative without ſome eſſential dates, and, I am ſorry to ſay, without probability.—Whatever ſubſequent diſeaſe the patient might experience, the evidence of the home-

* In this liſt, which is kept in ſeparate books by the phyſician's clerk, are ſpecified the name and age of the patients, the place of their abode, the name of the diſeaſe, the name of the recommender, the number of viſits, which are diſtinguiſhed as they are made by the phyſician or the clerk, and the time and manner of diſcharging the patients.—The value of theſe records has been often felt.

† Home-patients of the Mancheſter Infirmary are thoſe who are attended at their own houſes.—Out-patients are thoſe who are able to come for their medicines to the Diſpenſary on ſtated days, as in ſimilar inſtitutions.

patient-

patient-book muſt prove, that ſhe was able to walk abroad on the 27th of January, 1798.

I ſhall take this opportunity of obſerving, that I have received ſome very important facts reſpecting lymphatic inflammation, from ſeveral reſpectable practitioners, ſince the publication of my third volume.—Theſe ſhall be laid before the public as ſoon as my profeſſional avocations will permit.—They will confirm two points of my Eſſay, in which I have made an addition to the hiſtory of the diſeaſe: 1ſt. In obſerving the inflamed and enlarged ſtate of the trunks of the lymphatic veſſels; 2d. In ſhewing that it may take place in either ſex, conſequently independent of the circumſtances of parturition.—Mr. Trye's book, which has been mentioned as the ſource of my opinion, I have never read. I underſtood, however, at the time of publiſhing my obſervations, that he confined his views entirely to the diſeaſe in the puerperal ſtate. But it muſt appear to every candid perſon, that by diſcovering the actual enlargement and inflammation of the lymphatic trunks, in this diſeaſe, I have eſtabliſhed that as a theory, which was before only a plauſible hypotheſis. I derived my opinion from the evidence of my ſenſes, without obligation to any preceding writer, and my method of cure was directly deduced from that opinion.

It is with great reluctance that I thus come forward in my own vindication.—If it ſhall appear from this narrative, that the author of whom I complain has covered himſelf with diſgrace in the eyes of every man of honour, by his attempt to injure me, the fault is not mine. I am,

Dear Sir,

Your faithful and obedient ſervant,

JOHN FERRIAR.

Mancheſter,
Dec. 6, 1800.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

As the inoculation of the Cow-pox is known to be conducted upon a very extenſive ſcale at the Inoculation Hoſpital, and as the advantages which this new practice poſſeſſes are only to be learned from experience, I have hitherto judged it proper, from time to time, to furniſh the public, not only with the general reſults, but with moſt of the principal facts that have occurred to me in the propagation of this diſeaſe.—It appears, from

from my laſt publication on this ſubject, written about ſix months ago, that the number of perſons who had then received the vaccine infection at the Hoſpital exceeded 2500; ſince that time upwards of 1500 have been inoculated for the Cow-pox at the ſame place, and of theſe I have a report to preſent ſimilar to that ſtated by me in July laſt, viz. "With none of the patients did the infection occaſion a ſevere diſorder, or excite one alarming ſymptom."—The number of puſtular caſes under the Vaccine Inoculation, in the Hoſpital, has been even leſs than three or four out of an hundred, the proportion in which ſuch caſes were ſtated to occur at the period above mentioned. Reſpecting thoſe to whom I have communicated the infection out of the Hoſpital, or among my private patients, I have not yet met with one inſtance in which variolous-like puſtules took place. Indeed, I am convinced an eruption of that appearance will be found to be a very rare occurrence, unleſs, previouſly to the Vaccine Inoculation, or during its local progreſs, the patient has been expoſed to the action of variolous matter. Though ſuch an expoſure may not have been known, nor even ſuſpected to have taken place, yet this will not be deemed an objection of much weight againſt the opinion here advanced, when it is conſidered that the ſame obſervation will apply to four-fifths of all who caſually receive the Small-pox.—If a perſon who has been expoſed to the contagion of the Small-pox, for four or five days, be then inoculated for this diſeaſe, the inoculation anticipates, or prevents, the effects of the contagion, and the inoculated Small-pox is produced. But, if the Vaccine Inoculation be employed in a caſe thus circumſtanced, the Small-pox is not prevented, although the tumour produced by the inoculation advance to maturation. Hence we are to expect that the caſual Small-pox will more often ſupervene to the vaccine than to the variolous inoculation.

It was not before the commencement of the preſent year that I aſcertained the Cow-pox had not the power of ſuperſeding the Small-pox; for though, from the firſt trials I made of the new inoculation, it appeared that theſe diſeaſes, as produced in the ſame ſubject from inoculation, did not interrupt the progreſs of each other, yet as the caſual does not act in the ſame manner as the inoculated Small-pox, and may be anticipated by the latter, I thought it ſtill probable that the Cow-pock infection might have a ſimilar effect. Numerous facts have, however, proved this opinion to be unfounded, and that the variolous effluvia, even after the Vaccine Inoculation has made a conſiderable progreſs, have, in ſeveral inſtances, occaſioned an eruption reſembling that of the Small-pox. This

latter

latter effect of the Small-pox I did not conceive to be possible, till after I had made repeated trials of the new inoculation out of the Hospital; nor is the fact to be easily explained, when it is considered that the vaccine inoculation imparts its effects to the constitution in a shorter time than the latent period of variolous infection, which is commonly from the eleventh till the fourteenth day.

In those cases of Vaccine Inoculation in which the variolous infection has an early effect, I have observed that the tumour at the inoculated part proceeds slowly, and never exhibits any efflorescence; the pustules also are more numerous, when they appear early in the disease, than when they do not appear till after the twelfth day of the inoculation.

From the preceding observations we may infer, that in this metropolis, and its vicinity, where the Small-pox constantly, more or less prevails, the Vaccine Inoculation must sometimes be attended with a pustular eruption, of which it is not the cause. But inoculators, not adverting to this, have generally ascribed the eruption to a variolated state of the Cow-pock matter, with which the patient was inoculated; and the Inoculation Hospital has been commonly represented as the place in which this adulterated matter was generated and obtained. To refute this opinion I adduced several facts, proving that varioliform pustules had frequently accompanied the Cow-pock inoculation, though no doubt could be entertained of the genuineness or purity of the vaccine matter employed for the inoculation; and also a number of experiments, sufficient to shew that the Cow-pox does not *hybridise* with the Small-pox, but that both diseases continue distinct in the same patient; of which the following singular instance may be considered as an additional proof.

About two months ago, a girl, eleven years of age, was admitted into the Inoculation Hospital, where she was inoculated with vaccine matter; five days afterwards she was seized with the symptoms of Small-pox, and an eruption of pustules (about 200) took place. On the 10th day of the inoculation, one of the variolous pustules appeared distinctly within the margin of the vaccine tumour. I charged a lancet with matter taken from the centre of the tumour, and with it inoculated a child, in whom it produced a regular case of Cow-pox. Mr. Wachsel, the apothecary to the Hospital, who inoculated three children with matter taken from the pustule in the vaccine tumour, found that it communicated the Small-pox to all of them.

Ely Place, Dec. 1800. WILLIAM WOODVILLE.

To

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

IT has always appeared to me, that the publication of caſes, however important or authentic, ſhould be reſtricted to ſuch as tend to explain ſome remarkable fact, or determine ſome point of doctrine that is not perfectly ſettled. One of your correſpondents has lately advanced an opinion, that all diſtortions in the legs or feet, by whatever cauſe produced, *or how formidable ſoever they may appear*, may be perfectly cured by the application of one inſtrument. If this was ſo, it would materially ſhorten my labour in the communications I propoſe to ſubmit to you upon this ſubject, as I ſhould be authorized to conclude, that all diſtortions of the feet might certainly be cured by the very ſame means that were uſed to cure the patient whoſe caſe I related in my laſt; and by producing the ſpecific, put an end to all farther inquiry on the ſubject. But, as not one particle of the opinion, ſo confidently given, is true, I hope I ſhall be excuſed for ſending the hiſtory of another caſe of the ſame diſeaſe; by comparing which, with that you have already publiſhed, your readers will be competent to decide upon the truth and probability of that opinion.

There is another claſs of diſeaſes that is, in ſome reſpects, ſimilar to the above, viz. what are called contractions of the limbs, the conſequences of palſy, and other diſeaſes; as many of theſe may be cured by a ſkilful application of the ſame means that I have uſed in the cure of other diſtortions, I ſhall, in my next, relate a ſingular caſe of this deſcription, and then proceed to examine the theories that have been held, and the practice that has been founded on them, in attempting to cure theſe diſeaſes, and conclude with a full expoſition of my own.

I am, GENTLEMEN,

Your moſt obedient ſervant,

T. SHELDRAKE.

No. 50, Strand,
December 16, 1800.

P. S. Permit me to ſay, I ſhall ſend no anſwer to your correſpondent, Mr. Watt; for he has deſcended from diſcuſſing the ſubject in diſpute to *perſonal abuſe, and is,* THEREFORE, *fallen too low for me to notice.*

CASE II.

Sept. 7, 1798. A nephew of Mr. Duryer, No. 16, Dean Street, Holborn, was placed under my care for the cure of one

diſtorted foot; it happened before the birth; and from that period to the time I ſaw him, many unſucceſsful attempts were made to relieve him. He was, at this period, ſeven years old; the toes were of the natural form, but the whole metatarſus was diſtorted at its junction with the tarſus, and the foot was ſo twiſted from its natural poſition, that the toes pointed directly towards the oppoſite leg, and he ſtood upon a calloſity which had been formed on the outſide of the foot, upon the exterior cuneiform bone; the ſole of the foot was expoſed to view as he ſtood, and the whole foot was perfectly rigid, and the leg much waſted.

In four months the foot was ſo much altered, that it might be placed in its natural poſition while he ſtood upon it, but it was incapable of any voluntary motion; when he endeavoured to walk, it hung an uſeleſs appendage to the leg, and immediately fell into its original poſition.

In eight months more the foot was ſo far reduced to the natural ſtate, that he could walk upon it for a ſhort time in the natural way; but as it remained weak, and there was, after ſome time, a viſible tendency to relapſe, it was again bound up, and continued ſo for ten months more, when the bandages were laid aſide; and as, after ſix weeks trial, there appeared no tendency to relapſe, I finally took my leave of the patient.

It would be very eaſy to ſay, in few words, that this patient was completely cured; and thoſe, at leaſt, who ſhould ſee him walk flat upon this foot, and at the ſame time remember the ſtate it was in, would not diſpute the aſſertion; but as I think an accurate ſtatement of the facts will be more ſatisfactory, I truſt I ſhall be permitted to explain them.

I have one caſt taken from the foot in its originally diſeaſed ſtate; a ſecond, taken when it could be placed in the natural poſition, but remained flaccid and powerleſs, and therefore uſeleſs; a third, taken at the time he began to uſe the foot in the natural way; and a fourth, when I took my leave of him. By the aſſiſtance of theſe caſts, inſtead of referring to my own memory, or that of the child's friends, the one of which might be thought partial, and the other could not ſhew correctly what that diſeaſe was, which no longer exiſts, I am enabled to lay it before the reader in the moſt authentic and incontrovertible manner, by annexing correct drawings of the diſeaſe in its original, and the foot in its amended ſtate. The plate marked 3, contains four views of the diſeaſed foot; Fig. 1, is a front view of the leg, to ſhew how the foot was turned upwards and inwards with reſpect to it; Fig. 2, a back view of the leg for the ſame purpoſe; Fig. 3, a view of the outſide of the leg, in which no part of the metatarſus appears, becauſe it is turned directly

directly inwards; and Fig. 4, is an inside view of the leg, to shew the position of the toes in that point of view.

Whoever is acquainted with the structure and natural position of the bones of the foot, will perceive immediately upon inspecting these views, that there was no distortion of the bones of the metatarsus, individually, or with respect to each other, but that the metatarsus began to form an acute angle, turning directly inwards at its junction with the cuneiform bones: In these again, there appears to have been little distortion, either with regard to their mutual connection, or their connection with the astragalus; but the astragalus itself was so far removed from its natural situation, that instead of its circular head lying in the scaphoid cavity of the tibia, it lay in a direct perpendicular line with the fore part of the leg: the calcis, instead of being placed upon the ground, lay in contact with the back part of the tibia; and the scaphoid cavity of this bone rested upon those parts of the tarsus and the astragalus, by which they were joined together: the result of this combination was, that the patient stood, and could only walk upon a callosity which was formed upon the outermost cuneiform bone. Add to this, that the distortion took place before the birth, and by remaining in this state till the patient was seven years old, the whole mass was so firmly connected, as to be perfectly rigid, and no part of it seemed endowed with any capacity for locomotion: an attention to these circumstances will be necessary, when we come to consider the treatment. Plate 4, contains four views of the leg, taken from the cast which was made after I quitted the patient. In making these views, I placed the leg in the same positions as in drawing the figures, with corresponding numbers in the former plate; of course, a comparison of the two plates will shew exactly the difference between the foot in its diseased, and in its amended state.

I have only taken the extremes of this case, as the best method of shewing the full effect of the alteration produced; it would be needless to give drawings of the intermediate casts of the foot, though it may be proper to observe, that in the second state of the case, when the foot could be placed in the natural position, but had no power to retain itself in it, it fell nearly into the form of the original disease, but the skin remained loose, and was universally wrinkled; in the third state, it approached nearer to the natural form, but still it had some distinguishing peculiarities; the instep was much higher, the shape of the ancles was not so well defined, and there was much loose skin about the ancle and outside of the foot; this afterwards disappeared; and when the last cast was taken, the foot measured five-eighths of an inch more in length than

it

it did when the one before it was made. As this is more than can be accounted for from the natural growth of a naturally formed foot in the ſpace of ten months, the ſurplus muſt be attributed to the alteration produced in the general ſtate of the foot during the progreſs of the cure.

I have hinted that this foot cannot be ſaid, with propriety, to be *perfectly* reſtored to its natural ſtate. If Fig. 2 and 3, in Pl. 4, are examined, it will be ſeen that that part of the fibula which forms the outer ancle, is removed from its natural ſituation, and is quite cloſe to the tendo Achillis; though this peculiarity does not render the foot unſerviceable, it certainly will never occaſion the leg to be admired for its beauty, and in this caſe I believe it to have been irremediable, though I have ſucceeded in removing that, as well as every other defect in younger patients. I believe it muſt have happened in the following way.

The fibula, from its length and poſition on the ſide of the aſtragalus, guards the joints ſo ſecurely from luxation outwards, that Mr. Pott ſaid, it was an invariable rule that this joint could not be ſo luxated without fracture of the fibula; perhaps he may be right: but it is certain that in this and ſimilar caſes, where the diſtortion takes place while the bones are in a cartilaginous ſtate, the head of the aſtragalus, by puſhing forwards, firſt drives the fibula outwards, and then forces it backwards towards the tendo Achillis. To render the cure in this kind of caſe perfect, it ought to be undertaken while the bone is in a cartilaginous ſtate; in this ſtate I have ſeveral times cured it entirely: but in the caſe I have juſt related, I could not ſucceed in removing that particular defect, though it may not hereafter, in other caſes perhaps, prove irremediable.

Remarks on the external Uſe of cold Water in Fever; by T. Garnett, M. D. Profeſſor of Natural Philoſophy and Chemiſtry in the Royal Inſtitution of Great Britain.

NO greater improvement has lately taken place in medicine than in the treatment of Fevers, by the external uſe of cold water; a mode which was firſt, I believe, practiſed by Dr. Wright, but ſince elucidated by the acute reaſoning and confirmed by the experience of Dr. Currie. I have ſeveral times witneſſed, not without ſome degree of aſtoniſhment, the wonderful efficacy of this plan; and I only preſume to ſtate a few facts, with a view of drawing the attention of the faculty in

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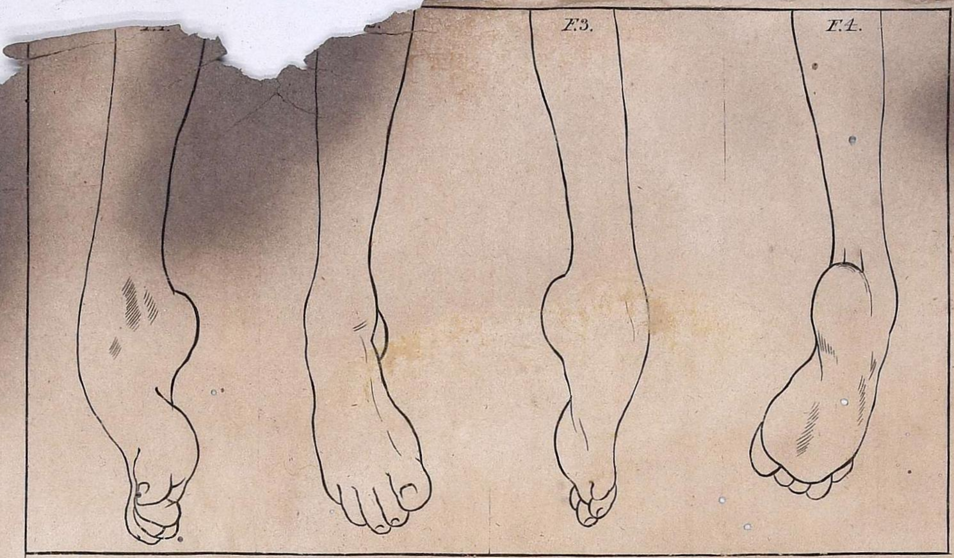
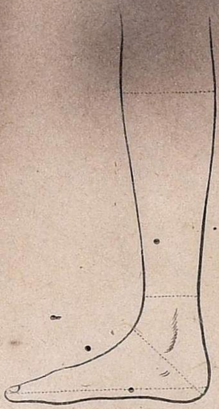

F.3.
F.4.

F.1



F.2.

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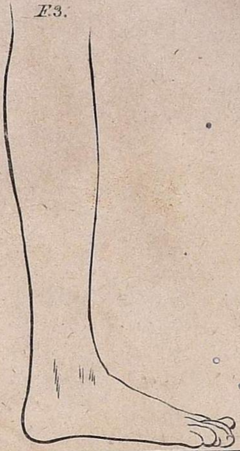

F.4.

this city to this point; for, though the plan is ufed by fome very judicious practitioners, it is by no means general. I am, however, convinced from experience, that there are few cafes of *typhus* in which the pulfe exceeds 100, where the fkin is dry, and its heat confiderably above the natural heat of the human blood (circumftances which ought to be diligently attended to), where a cure may not fpeedily be effected by it, provided it be employed within the firft fix or eight days from the attack of the fever. Among many other cafes I fhall only give the following:

I was defired to vifit Mr. T. a refpectable manufacturer in Glafgow, who laboured under a mild kind of typhus, from which, however, he did not recover fo quickly as was expected. It was on the twentieth day of the fever that I faw him, and I prefcribed for him the oxymuriat of potafh, a remedy which I have been in the habit of ufing in this complaint for fome years with great fuccefs. He grew better daily; but before he was perfectly recovered, Mrs. T. who had given him almoft conftant attention, was attacked with fymptoms of fever; I did not fee her till the third day after the attack, when I found her pulfe not lefs than 130; the heat of her fkin 160°, without any moifture; her eyes had a confiderable degree of wildnefs, and her tongue was quite brown and parched. In fhort, I think I had never feen the difeafe at the fame period attended with worfe fymptoms.

As this feemed a cafe exactly adapted to the plan laid down by Dr. Currie, I propofed it to the furgeon who attended her along with me, a very intelligent practitioner, who immediately affented to it. She was directed to be taken out of bed, and placed in a large tub on a ftool, and a large bucket of cold water, in which about a pound of common falt had been diffolved, was thrown upon her; her fkin was immediately wiped dry, and fhe was put to bed. In about ten minutes afterwards I went into the room, and found the pulfe 94; the heat of the fkin 96°. On afking her how fhe felt herfelf, fhe replied, as well as ever fhe was in her life: I left her, directing the affufion to be had recourfe to again if the febrile fymptoms returned during the night. I faw her the next morning, and found her without any fever, of which fhe had no return.

Another patient on whom I tried this remedy, was Mifs R. aged 17. She had taken the infection from her brother, who had recovered very flowly from a typhus of the worft kind. She was attacked with fhivering, fucceeded by great heat, and pain in the back; her pulfe was 128; the heat of the fkin 102°, and dry; her tongue parched and brown, though not quite fo bad as in the former cafe. The cold water was ufed in

in the ſame manner, and in a quarter of an hour the febrile ſymptoms had vaniſhed; the pulſe was under 100, and the heat of the ſkin natural. The febrile ſymptoms, however, returned in about ſix hours, but went off on repeating the affuſion; they returned again in the courſe of the next day, but were again overcome; ſhe was obliged to uſe the affuſion five minutes; but at laſt, after forty-eight hours from the firſt affuſion, the complaint had totally left her.

Two days after this, another brother of Miſs R.'s was ſeized with ſymptoms of fever, in which the heat of the ſkin, pulſe, and tongue, was much the ſame as in his ſiſter's caſe; and he was delirious, conſtantly wiſhing to go to the grammar-ſchool to receive a prize to which he was entitled. The affuſion was uſed in the ſame manner as in the preceding caſes; the febrile ſymptoms immediately vaniſhed—the delirium left him—his pulſe and heat became natural—he fell into a ſound ſleep—perſpired profuſely; and in the morning found himſelf ſo well, that I am pretty ſure he did go to the grammar-ſchool to claim his prize.

Royal Inſtitution, Dec. 1, 1800.

Account of Diſeaſes in an Eaſtern Diſtrict of London, from the 20th of October to the 20th of November, 1800.

ACUTE DISEASES.	
Typhus - - - - - -	25
Peripneumonia Notha -	8
Dyſentery - - - - - -	7
Cynanche Tonſillaris - -	5
CHRONIC DISEASES.	
Cough - - - - - - -	25
Dyſpnœa - - - - - -	10
Cough with Dyſpnœa -	12
Hydrothorax - - - - -	5
Hæmoptyſis - - - - -	3
Pleurodyne - - - - -	2
Diarrhœa - - - - - -	15
Enterodynia - - - - -	5
Chloroſis - - - - - -	4
Amenorrhœa - - - - -	12
Hyſteria - - - - - - -	3
Hypochondriaſis - - - -	7
Hemiplegia - - - - -	2
Cephalalgia - - - - -	7
Vertigo - - - - - - -	5
Epilepſy - - - - - -	3
Herpes - - - - - - -	14
Lumbago - - - - - -	2
Chronic Rheumatiſm - -	15
PUERPERAL DISEASES.	
Low Puerperal Fever - -	5
Abſceſſus Mammæ - -	1
Rhagas Papillæ - - - -	3
Iſchuria Veſicalis - - - -	1
INFANTILE DISEASES.	
Dentitio - - - - - -	3
Diarrhœa - - - - - -	7
Convulſio - - - - - -	2

The preſent reigning epidemic is a fever of the typhoid kind, which has proved highly alarming in its ſymptoms, and frequently fatal in its termination. Several medical practitioners, by an aſſiduous attention to the ſick, have become infected by it; and whilſt attempting

attempting to relieve the diftreffes of others, have fallen victims to the difeafe. In fome inftances this fever has proceeded very rapidly to a fatal crifis, whilft in others its progrefs has been more flow. It has generally commenced with chillinefs and laffitude, to which have fucceeded an increafed degree of heat, quicknefs of pulfe, and great proftration of ftrength. Giddinefs has very frequently occurred, and this has been the firft fymptom which has occafioned any alarm to the patient or his friends. This fever has generally been attended with great determination to the head, indicated by a flufhed countenance, ferrety eye, a confiderable degree of pain, and in fome inftances by very early delirium. In one of the cafes that occurred to the reporter, the delirium did not occur till a late period, when it proved to be of the moft active and furious kind, and rendered abortive every attempt to remove the difeafe.

Several inftances have occurred of that fpecies of fore throat which has been denominated Cynanche Tonfillaris. In this difeafe, befide other febrile fymptoms, there is an inflammation of different parts of the fauces, but particularly of the tonfils and uvula. Thefe parts become red and tumid, by which a painful deglutition is occafioned, fometimes accompanied with a pain fhooting to the ear. In one of the cafes referred to, there was an unufually large fecretion of mucus, the difcharge of which from the mouth occafioned confiderable fatigue and inconvenience to the patient. By purfuing the antiphlogiftic plan, with moderately aftringent applications to the inflamed parts, and the application of rubefacients to the neck, the difeafe was gradually fubdued.

Account of Difeafes in an Eaftern Diftrict of London, from the 20th of November to the 20th of December, 1800.

	No. of Cafes.
ACUTE DISEASES.	
Typhus	15
Sore Throat	3
Peripneumony	2
Dyfenteria	1
Small-pox	3
CHRONIC DISEASES.	
Cough	18
Cough with Dyfpnœa	17
Dyfpnœa	12
Hæmoptyfis	3
Catarrh	7
Phthifis Pulmonalis	4
Gaftrodynia	8
Hæmatemefis	1
Dyfpepfia	7
Diarrhœa	10
Enterodynia	4
Colica	2
Epilepfia	2
Hyfteria	6
Apoplexy	1
Hemiplegia	2
Hypochondriafis	3
Chlorofis	5
Fluor Albus	8
Amenorrhœa	7
Chronic Eruptions	15
Lumbago	2
Chronic Rheumatifm	20
PUERPERAL DISEASES.	
Ephemera	4
Milk Fever	2
Menorrhagia Lochialis	3
Maftodynia	7
INFANTILE DISEASES.	
Aphthæ	3
Convulfio	4
Ophthalmia	2
Ophthalmia Purulenta	4

The

The fever, which a few weeks ago was considered as the prevailing epidemic, though the violence of its symptoms has abated, still continues to engage the attention of medical men. Violent affections of the head, the symptom by which it was particularly characterized at its first appearance, occur less frequently; but the inflammation and slight ulcerations of the throat, which were taken notice of in the last report, still continue very frequently to attend the other symptoms.

We have now entered upon that season of the year in which affections of the lungs form a very considerable proportion in a list of diseases. Cough and dyspnœa, inflammation of the lungs, hæmoptysis and catarrh, are perpetually requiring medical assistance.

These complaints, it is well known, prove troublesome and tedious, and, in constitutions which are subject to them, very frequently continue in a greater or less degree of aggravation through the winter. An early attention to the symptoms of these complaints might, in some instances, produce a speedy termination: but what is commonly called a cold, is considered as a disease too frequent and too slight to require any particular notice, especially amongst persons engaged in business, who consider the confinement and absence from their usual occupation, as greater evils than the disease itself: thus it is left to take its chance, till, in some patients, it degenerates into an inflammation of the lungs, and terminates in a confirmed phthisis pulmonalis.

Diseases admitted under the care of the Physicians of the Westminster Hospital, from the 22*d of October to the* 26*th of November*, 1800.

	No. of Cases.		No. of Cases.
Continued Fever	24	Hæmoptysis	1
Intermittent	1	Hysteria	3
Pleurisy	2	Itch	7
Amenorrhœa	2	Lichen	1
Anasarca	2	Lithiasis	1
Apoplexia	1	Lumbago	1
Ascites	2	Obstipatio	1
Catarrh	8	Paralysis	2
Cholera	1	Phthisis	1
Cough, &c.	14	Pleurodyne	1
Diarrhœa	3	Rheumatism	9
Dyspnœa	4	Syphilis	2
Enterodynia	2	Vertigo	2
Epilepsy	1	Vomitio	1
Erythema	1	Worms	1
Gastrodynia	1		

Cautions reſpecting the Inoculation of Cow-Pox; by Dr. Stokes.

To Dr. BRADLEY.

Dear Sir,

THE diſcovery of a diſeaſe in a quadruped of a genus ſo totally diſtinct from man, which, without exciting any conſiderable diſturbance in the conſtitution, renders him inſenſible to the action of the moſt generally fatal malady to which he is ſubject, is a phænomenon which has arreſted the attention of every enlightened phyſiologiſt; and the reception which this diſcovery has met with, deſerves the attention of the metaphyſician. The common people, as far as I have obſerved, receive it with gladneſs, as it comes to them free of expence, and ſome of them have been heard to ſpeak of it as a bleſſing ſent from God. They do not expreſs any more repugnance to the inſertion of pellucid lymph from the arm of a neighbour's child into that of their own offſpring, becauſe originally taken from a veſicle on the nipple of a cow, than they do to feed them with the milk or fleſh of that animal. Their minds are no longer held in ſuſpenſe, whether they ſhall conſent to give their child a malady which may terminate in its death, with a view to preſerve it from a diſeaſe which they ſometimes obſerve to be a very ſlight one. The inoculator can now aſſure them, that their child *will not die of inoculation*, and every new inoculation confirms to them the truth of his aſſertion. The middling claſſes, who are in the habit of being guided by the practitioners they employ, take their opinion on the ſubject, and ſometimes commiſſion them to inoculate their children "with either kind of pock." The upper claſſes, who read, and ſee a variety of practitioners, judge for themſelves; and thoſe among them who are philanthropiſts and converts to the new faith, inoculate their own children and thoſe of the poor together. Mr. Fenner's pamphlet will, I doubt not, make many converts among this claſs, and I hope his example will have many followers. With regard to our medical brethren, the metaphyſicians will tell us, that we are not in general to look up for ſupport from the timidity of advancing years, and they will forbid us to be ſanguine in promiſing ourſelves the ſanction of thoſe who are paſt 70. I have, however, the pleaſure to obſerve, that Dr. Hunter, of York, in a ſpeech delivered before the Directors of the Diſpenſary in that city, recommends a general vaccine inoculation, with all the ardour

ardour and enthuſiaſm of youth. Other veterans in medicine will, I doubt not, ſtand forward in the ſame cauſe; and ſhould we live to ſo advanced a period of life, may we prove equally alive to the benefits likely to be derived from ſome other diſcovery equally beneficial to mankind! But the moſt formidable claſs of opponents which the new inoculation has to encounter, are thoſe who calculate their future income by the quantum of expected diſeaſe; yet theſe we might ſoon make converts to the new doctrine, if we could convince them that the rapid increaſe of population would amply compenſate for the apprehended defalcation of their accuſtomed profits. But the patrons of the new inoculation need to fear no oppoſition, if in ſteadily purſuing the practice they ſtudy Nature, and guarding themſelves againſt the ſeduction of falſe theory, hold themſelves ever ready publicly to acknowledge any errors into which they may have been led by inaccurate or limited obſervation. They ought to keep up a conſtant ſucceſſion of inoculations, ſo that recent matter may be always in readineſs; but if the Small-pox ſhould break out in any family, all thoſe who have not had the diſeaſe ſhould be inſtantly inoculated with Cow-pox virus, even though labouring under other diſeaſes; the parents being at the ſame time reminded, that the Cow-pox is no preſervative againſt ſuch diſeaſes, nor even againſt Small-pox, provided the ſubject of inoculation has been expoſed to its miaſma. In ſuch caſes the inoculator ſhould make ſeveral punctures, and the patients, if convenient, ſhould be inſtantly removed to a ſituation where they may not be expoſed to the contagion of Small-pox: Additional ſecurity may be obtained by making a freſh inoculation every day till marks of infection appear. Some of the beſt writers on the ſubject, ſpeak of the difficulty of communicating the diſeaſe, and ſeem to think it a virus leſs active than that of Small-pox. My experience has led me to draw an oppoſite concluſion. My method is to introduce the lancet into the Cow-pox veſicle. In a few moments after withdrawing it, a ſmall pellucid globule appears on the cuticle, which I ſcrape off by the edge of the lancet; I then turn the lancet, and ſcrape the cuticle ſtill wet with the other edge; I then introduce the lancet between the cuticle and cutis of the left arm of the patient who is to be inoculated, holding the ſide of the lancet parallel to the arm, the arm hanging in its natural poſition, but the point of the lancet perpendicular to the horizon, that the virus may flow downwards into the minute wound; and its introduction I endeavour to facilitate by gently moving the lancet upwards and downwards. I ſometimes turn the lancet, and introduce it into the ſame punctures, which are generally viſible, in children at leaſt, whoſe cuticle is ſo thin,

that

that it is difficult to introduce the lancet without some slight effusion of blood. These precautions are peculiarly necessary where the Small-pox has already made its appearance in the family. In two instances where the patients were inoculated with recent virus in the afternoon of the day on which the Small-pox broke out in another child in the family, no pustules appeared excepting those on the arm; but in Job Webster, two months old, inoculated in both arms with virus taken on a lancet and carried a quarter of a mile, pustules appeared on the 4th day on the lower limbs, at the same time that one of the punctures became elevated into a red pimple, which I saw on the 4th, but it was not till the 6th that I saw the pustules: the inoculated pustule was now become orbicular, with a slight depression in the middle. On the lower limbs and the lower part of the trunk we counted sixteen pustules. The largest, which was on the right knee, was orbicular, and depressed, exactly as that on the arm, but rather larger, and surrounded by an efflorescence of very minute papillæ, similar to what I have sometimes observed round pustules on the arms of Cow-pox patients. The child's mother, one of my female pupils, and Mr. Walker, surgeon, who is in the habit of inoculating with both Small-pox and Cow-pox, were of opinion with me, that it was a Cow-pox pustule, and I became for a time a convert to the opinion of Dr. Woodville, that these pustules were the consequence of the action of a variolated atmosphere. On the 7th, the pustule on the arm measured in diameter 1 ½ tenth of an inch, and was surrounded by a very slight redness; that on the knee was 2 ¼ tenth of an inch, and was of a dirty brownish hue in the middle. There was a lenticular pustule on the left temple, 1 ¼ tenth of an inch in diameter, and two or three on the lower part of the abdomen, which were of a whitish hue in the middle. The rest on the body resembled that on the knee, excepting two on the middle of the abdomen, which appeared like variolous pustules. The lower limbs and the inferior part of the trunk were of a redder hue than the rest of the body, similar to the appearance of the skin in scarlatina. On the eighth day, the pustule on the arm was increased to two-tenths of an inch, and the surrounding skin was more inflamed. The pustule on the knee was browner in the middle, and the surrounding minute pustules larger; and more pustules made their appearance on the upper part of the body. The point of a lancet was passed into the margin of the pustule on the knee, and into some of the surrounding pustules; but neither lymph nor pus exuded. The lancet was barely moistened, and being carried to the distance of about a hundred yards, was inserted into the arm of a child four years old. Another lancet was intro-

duced into the puftule on the arm, but neither lymph nor pus exuded. The moiftened lancet was carried to the fame diftance, and inferted into the arm of another child, 1 $\frac{1}{2}$ year old. On the ninth, the puftule on the arm was furrounded by an inflamed areola of about an inch in diameter; that on the knee was larger, as alfo its furrounding minute puftules, and more puftules had appeared on the upper part of the body, and one or two on the face. On the tenth, the puftule on the arm was larger, but the inflammation round it as the day before; thofe on the limbs were rather larger, and more had appeared on the face. On the eleventh, the puftule on the arm meafured 3 $\frac{1}{2}$ tenths of an inch, and was of a whitifh afh colour, and the areola was become rather paler; thofe on the knee were covered with a yellow cruft.

Elizabeth Titley, the child *inoculated from the puftule on the arm* of Job Webfter, went through the Cow-pox in the ufual way. I was informed that on the feventh day the punctures inflamed, and I examined the arm on the eleventh; the puftule meafured 3 $\frac{1}{2}$ tenths of an inch, and the areola 1 inch eight-tenths in diameter. On the twelfth, the greater part of the areola had nearly difappeared, but it had extended with an irregular outline towards the axilla. The puftule afforded pellucid lymph.

In Hannah Titley, the child *inoculated from the puftules on the knee* of Job Webfter, I was informed the punctures became inflamed on the ninth, two days after her fifter's. On the eleventh, when I firft faw the child, the puftule on the arm meafured 2 $\frac{1}{4}$ tenths of an inch, was depreffed in the middle, and was furrounded by an inflamed fpace half an inch diameter, of a deep red. Her mother obferved, that fhe had been hot and not well for two or three days, and that fhe had eaten very little. In the evening a puftule appeared on her right arm. On the twelfth, the puftule on her right arm appeared elevated, with a flight depreffion at the top, and others had appeared on all parts of her body. She took little or no food, and her pulfe was 120. On the thirteenth, the puftule from inoculation appeared irregular, with an angular margin, with feveral fmall ones round it, and more puftules had come out. On the fixteenth, the puftule being opened by a needle, a pellucid lymph exuded. The puftule and inflammation round it meafured 1 inch 1 tenth in length. On the feventeenth, the puftule was fpread wider and longer, with an irregular margin, exactly like that of inoculated Small-pox; and on opening it with a needle, the contents were a ftraw-coloured opake fluid. The inflamed fpace furrounding it was very narrow in proportion. The puftules on the body were conical, rather obtufe at the end; fome contained pus at the extremity, and others

others were covered with a yellowiſh-brown ſcab. On the eighteenth, the child was playing out of doors.

Hence it is evident, that the puſtules on the body of Job W. were variolous, probably derived from the ſame ſource as thoſe of the patient in whom the diſeaſe broke out four days before him, and that the puſtules on his arm were pure Cow-pox, both diſeaſes going on at the ſame time; the lancet inſerted into the puſtule on the arm producing genuine Cow-pox in Elizabeth Titley, and that infected by the puſtules on the knee, Small-pox in Hannah Titley. But I am inclined to believe, that the Cow-pox inoculation modified the progreſs of the Small-pox, which, inſtead of appearing on the face and boſom, broke out on the lower extremities, proceeding very gradually upwards.

I have ſeen the two diſeaſes exiſting together in three other inſtances. Mary Waſſe, of Aſtwith, 5 years old, after having juſt gone through the chicken-pox, was inoculated by her mother, a very intelligent woman, in both arms, with Cow-pox virus, taken by her huſband on a lancet, two or three days before, from a patient inoculated by a cabinet-maker about three miles diſtant, on the 24th of July. On the 6th day, one of the punctures appeared inflamed, and ſtill more ſo on the 7th, in the evening of which day ſhe complained of not being well. On the 8th ſhe had no appetite, could not walk, and was feveriſh in the afternoon; and on the 9th the Small-pox broke out in the face. During all this time the inoculated puſtule proceeded regularly, and on the 11th appeared to be at the height. The mother, from whom I received the above account, which I wrote down on the ſpot, and who had inoculated one of her children ſome years before with Small-pox, deſcribed the inoculated puſtules as being circular, lower in the middle than at the edge, and as appearing to contain a fluid of a bluiſh white. On the 14th day, the 6th day of the eruption of Small-pox, I firſt ſaw the patient. There was a degree of redneſs in the ſkin round the inoculated puſtule, ſuch as remains after Cow-pox, and the puſtule now ſecreted ſome yellow pus at the margin, as Cow-pox puſtules ſometimes do. There were no Small-pox puſtules within the limits of the faded areola. The Small-pox I was informed went through its uſual courſe, and ſome weeks after I ſaw the child perfectly recovered. The Small-pox had prevailed in the village ſome time, and I doubt not, this child had received the infection previous to the inſertion of the Cow-pox virus.

W. Drabbles, of Inman-Thorpe, a year and a half old, was inoculated by Mr. Walker, ſurgeon, of this town, with Cow-pox virus, on the 6th day preceding that on which the Small-pox broke out. On the 3d day of the Cow-pox inoculation, the

the punctures appeared inflamed. I ſaw the child on the 8th day. They were true Cow-pox puſtules, but ſeveral Small-pox puſtules had made their appearance within the limits of the areola; and on the 12th day, the 8th of the Small-pox fever, the child died.

Another child of the ſame family was inoculated with Cow-pox virus, on the 4th day preceding that on which the Small-pox broke out, and the day before the commencement of the variolous fever. On the 3d day the punctures appeared inflamed, and on the 8th there were two Cow-pox puſtules, with one of Small-pox, at the diſtance of half an inch from them. Mr. Walker informed me, that on the 12th day, the 11th of the Small-pox fever, the puſtules looked all alike, the Cow-pox puſtules not being ſurrounded by an areola. This patient recovered.

Hannah Drabbles, 6 years old, was inoculated with Cow-pox, on the 7th day preceding that on which the Small-pox broke out. On the 8th day, one of the punctures for the firſt time appeared ſlightly inflamed, and Mr. Walker informed me, that on the 12th, the 8th of the Small-pox fever, they all appeared exactly ſimilar to the Small-pox puſtules. This child alſo recovered.

The next caution I have to mention is, that all inoculators ſhould ſtrenuouſly inculcate the neceſſity of every patient being inſpected at ſuch periods of the diſeaſe, as may enable them to pronounce whether the patient is rendered ſecure from the action of Small-pox. At Ilkeſton near Nottingham, two children were inoculated with Cow-pox virus. Eight weeks after they caught the Small-pox, and one of them died. Being in the neighbourhood, I extended my ride to inquire into the circumſtances. Mr. Williams, the ſurgeon who inoculated them, carried me to the parents, from whom I learnt that the puſtules roſe in the courſe of a few days into elevated puſtules, like thoſe of diſtinct Small-pox, were never flat, were ſoon changed into a ſcab, and that the ſurrounding ſkin was not inflamed to more than half an inch from the baſe. I drew repreſentations of convex and depreſſed puſtules, viewed in profile. The mother ſaid they were like the convex, but never like the depreſſed. Hence it was evident, that the diſeaſe which the Cow-pox virus excited, was the cruſtaceous puſtule deſcribed by Dr. Jenner,* and which Dr. Woodville, in his excellent Obſervations on the Cow-pox, has proved to afford

* Continuation of Facts and Obſervations relating to the Variolæ Vaccinæ, 1800, p. 35.

afford no ſecurity againſt the Small-pox. The gratification I felt at having diſcovered the cauſe of the failure could only be equalled by the ſurpriſe I experienced, at finding the new inoculation in high reputation in the village. They were all inoculated with Cow-pox. I earneſtly recommended a re-inoculation of all the caſes which had not been regularly inſpected, and whoſe appearances had not been diſtinctly recollected, leſt the popular enthuſiaſm in favour of the new inoculation ſhould be damped by future failures.

The diſcovery of the cruſtaceous Cow-pox, (or *vacciola leproſa*, as I propoſe to our Noſologiſts to have it ſtyled, diſtinguiſhing the preſervative againſt Small-pox by the appellation of *vacciola ſcatellata,*) will enable us to reconcile the contradictory accounts of the effects of the caſual Cow-pox in ſecuring the conſtitution againſt Small-pox, as related by Drs. Jenner and Pearſon, and the caſes publiſhed in oppoſition by Drs. Ingenhouſz and Beddoes. It is natural to ſuppoſe that, in ſome inſtances, the ulcers on the hands and arms aſſumed the cruſtaceous form; and the contradictory evidence afforded by tradition, probably prevented the earlier introduction of the practice of Cow-pox inoculation.

Whenever any patient is reported to have the Small-pox after inoculation with Cow-pox virus, the inoculator ſhould take the earlieſt opportunity of inſpecting the caſe. J. Burton, who had the Cow-pox in July, had the chicken-pox about a month ago. I am,

Dear Sir,

Yours, &c.

Cheſterfield, Nov. 17, 1800. JONATHAN STOKES.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

FROM the communications upon the Cow-pock, a ſubject intimately connected with the intereſts of humanity, and deſervedly exciting the attention of the medical world, I think the Medical Journal has been of the greateſt utility; and it was with the greateſt pleaſure that, in the laſt number of your Journal, I read the remarks of Sir Chriſtopher Pegge of Oxford, and of Dr. Barry of Cork, upon the ſame ſubject. It appears from Sir C. Pegge's obſervations, that horſes are liable, in their heels, to the ſame diſeaſe as affects the teats of cows;

cows; from which fact, the suggestion of Dr. Jenner, concerning the occasional origin of the disease, would seem to be established; and perhaps further observations may shew, that other animals are not exempt from the action of this morbific matter. And Dr. Barry has rendered it probable from traditional testimony, that a disease affecting cows in a similar manner to the Cow-pock, and from them communicable to the human subject, has been known in Ireland for a long time; it would seem also, that this affection was more general and frequent, in former times, than at present.

Among the Irish, as we are told by Dr. Barry, this disease is known by the name of Shinach; a term, I presume, of Celtic extraction, and belonging to the Galic language. And feeling the importance of etymological inquiries, when cautiously pursued, in illustrating the manners, customs, and opinions of antiquity, and in reviving the primary signification of terms, I was induced from curiosity, and without any pretension to a knowledge of the Galic language, to look over in a cursory manner, some collections and remains of that Celtic dialect, which I found among my other books; and I was much surprised and gratified in observing, that it is very probable the word SHINACH is a compound Galic term expressive of a Cow's Teat; for from these collections I find, that the word SINNE, in Galic, means a teat, and the word AGH, in the same language, is a common term for a cow: So that it would seem, that SHINACH was originally written SINNEAGH; whence it will be readily admitted, by every person in any degree acquainted with the conversions and changes taking place in the history and progress of language, that in subsequent periods, the compound term SINNEAGH was variously pronounced and written, as Sinnagh, Sinnach, and Shinach. Similar instances to the contraction of Sinne into Sin, in the first member of the compound, are so frequent in all languages, that it is unnecessary to cite examples of it; and the conversion of the soft guttural consonant G, in the last member, into the hard guttural consonant C, as well as the insertion of the aspirate H, in the first syllable, are changes so familiar and conformable to the received principles of etymology, that I shall dwell no longer upon them.

These remarks, although avowedly more literary than practical, seem to me of no inconsiderable importance, as they shew, if just, that the knowledge of the existence of the Cow-pock has been coeval with the language of the very early settlers in Ireland; and it would appear from them, that among the ancient Irish, in remote antiquity, this disease was not only well understood to affect cows, but that its influence over the human

 man

man body was known and eſtabliſhed, as the name given it, when exhibiting itſelf in the human ſubject, was the term in the ancient language, characterizing the part of the animal whence the diſeaſe was derived; ſo that a perſon having this diſeaſe was ſaid to have the Shinach or Cow's Teat. And this mode of transferring the names of cauſes to their effects being the moſt natural, is the moſt eaſy proceſs of the human mind in the advancement of knowledge. Many terms of every ſcience may ſerve as documents of this kind of nomenclature; but to confine ourſelves to ſimilar inſtances of diſeaſes, named either immediately from their cauſes, and from ſome reſemblance to things external, or the effects of things external to the body, we need only turn over the common ſyſtems of noſology; where we ſhall find Raphania, a diſeaſe named from its ſuppoſed connection with a plant as its cauſe; Frambœſia, another diſeaſe, named from its likeneſs to a fruit; Urticaria, from its ſimilarity to the effects of a nettle; and Elephantiaſis, from ſome reſemblance, either to the natural form, or to ſome diſeaſed ſtate of the members of the elephant.

Theſe remarks, which may ſeem fanciful to ſome, I ſhall not extend any farther, but ſhall leave them to be confirmed or confuted by perſons having greater opportunities of inveſtigating their truth, from a greater knowledge of the Celtic language and its dialects, or from a more frequent intercourſe with perſons uſing that language. I am,

GENTLEMEN,

Your obedient ſervant,

RICHARD LUBBOCK.

Norwich, *Nov.* 23, 1800.

To Dr. BATTY.

MY DEAR SIR,

YOU will oblige me by inſerting the following Letter, on ſome irregularities I have obſerved in the Cow-pock, in your next Number of the Medical and Phyſical Journal. I fear that want of care in taking the vaccine matter at a proper period, and watching the inoculation through its progreſs, will bring ſome temporary diſcredit on the new inoculation in this neighbourhood. I have obſerved ſome ſuch inſtances with regret, and have heard of more; I therefore think that every effort to awaken a proper attention to this invaluable diſcovery, however feeble, may ſerve the great purpoſe of exterminating the Small-pox. That terrible diſorder is now prevailing with uncommon

uncommon ſeverity in York. Three children have lately been inoculated with Cow-pox matter, who, in a few days after, ſickened from previous infection of the Small-pox, one on the third day, another on the fourth, and the laſt on the fifth. One is dead, the other two are ſtill under the diſeaſe, but with hope of recovery. The Cow-pox veſicle formed in all, but ſoon loſt its characteriſtic appearance; its progreſs ſeemed to be arreſted at the period at which it was overtaken by the Small-pox.

I am ever yours affectionately,

R. CAPPE.

To the Editor of the York Herald.

Sir,

I have lately heard that in ſome inſtances perſons inoculated for the Cow-pox have immediately removed from the obſervation of the perſon who performed the operation: As ſuch want of care may lead to diſappointment and danger, I think the public ſhould be apprized of the neceſſity of attending to the progreſs of inoculation in its earlier ſtages; for it is only during the early period that the true Cow-pox can be diſtinguiſhed from an irregular effect of inoculation which does not afford ſecurity againſt the Small-pox.

I feel more ſtrongly, that the continued attention of perſons well acquainted with the true character of Cow-pox is neceſſary, from having lately watched with minute care an irregular caſe, in which on the fourth day after inoculation a puſtule aroſe; on the fifth day it became turgid and broke; yet on the ſixth day it had not entirely diſappeared, and on the ſeventh ſome fluid might ſtill have been obtained by preſſure. The puſtule never aſſumed the true character of Cow-pox: it contained a thin, yellow, muddy fluid, like diluted pus; but, at a late period, this inoculation could not be diſtinguiſhed from that which is a known ſecurity againſt the Small-pox. This puſtule would undoubtedly have deceived an inexperienced eye; and if the deſcription of it given by the parents had been relied on, it is probable that the child would generally have been thought ſecure againſt the Small-pox. Such imaginary ſecurity would unjuſtly bring diſcredit on the operation, and might prove fatal. It is ſurpriſing that no diſappointments of this kind have yet happened; none at leaſt have been made public; though irregular caſes probably have fallen under the obſervation of many inoculators, and there may have been others among thoſe inſtances, in which the progreſs of the diſeaſe has not been attended to: in the latter

 inſtance

inſtance it cannot be ſuppoſed that any ſubſequent care has been taken to prevent the Small-pox.

What proportion the irregular caſes may bear to the regular is not yet known. Among 115 patients under my own care, I have ſeen nine irregular caſes. I am diſpoſed to believe this proportion uncommonly great. The peculiar circumſtances in which three of them occurred, I have ſtated in your Herald of the 4th of October, with the caution they ſuggeſted.* Three others certainly aroſe from violence done to the inoculated part. Some of the remaining three might be fairly expected, ſince it is obſerved that one perſon in ſixty of all born,—ſome rate it as one in twenty,† is by natural conſtitution incapable of receiving the Small-pox, and is ſaid to be equally exempt from the Cow-pox.

It has been ſtated, that when inflammation appears on the inoculated part before the third day, the Cow-pox will not be produced.—This is not invariably true, though an early inflammation, from whatever cauſe it may ariſe, ſeems often to prevent abſorption of the matter, and when it does not prevent the Cow-pox aſſuming the true character, it ſometimes diſturbs the progreſs of the diſeaſe. Children have, at different periods of the inoculation, ſcratched or rubbed the inciſion:‡ if this happened when the veſicle was beginning to riſe, or if the whole ſurface of the veſicle was removed, the diſeaſe loſt its true character. When the veſicle was not entirely deſtroyed, the diſeaſe became irregular in its progreſs, an effloreſcence appearing earlier than uſual. When the inciſion was much rubbed before the period at which a veſicle ſhould begin to riſe, no veſicle was formed, but an extenſive circle, of an almoſt evaneſcent purple, appeared around the inciſion, and a ſoft paſty cruſt was formed upon it.

When a veſicle was torn at a late period, it ſometimes occaſioned a ſore; but this conſequence was often prevented by applications, that coagulating the fluid, which is copiouſly diſcharged, form a complete defence againſt the air.

I may add, as being immediately connected with this part of the ſubject, that when matter is taken for inoculation, it ſhould be done in the moſt delicate manner; for if the veſicle be much injured, the eryſipelas inflammation will be rendered ſevere.— Though

* Viz. Not to blend matter from two patients. Inconvenience may ariſe even from puncturing a veſicle with a lancet not perfectly cleaned from matter of another patient.

† Sauvage and Roſen von Roſenſtein.

‡ I have ſeen the danger of injury being done to the inciſion by nurſes not ſufficiently careful in gathering up a child's ſleeve.

Though the matter do not flow plentifully at first, on making a slight puncture in the vesicle, it will, in a little time, come in sufficient quantity to inoculate many persons. It seems partly supplied by secretion; for I have seen more matter obtained in a quarter of an hour than the vesicle could be supposed to contain. The matter flows slowly, as the vesicle is not one uninterrupted cell, but consists of many minute cells, which have so imperfect a communication, that they cannot be entirely emptied by a single puncture. During the formation of the vesicle, minute cells may often be seen arising separately, and they sometimes remain distinct on the margin. At a late period, when a crust is formed, a ring of clustered cells sometimes rise around it, more transparent than the original vesicle. These would probably afford matter for inoculation; for Dr. Woodville lately obligingly informed me, that at the request of parents, he had twice inoculated on the thirteenth day from patients, in whom the disease was so far advanced that he could only obtain a little moisture from the margin of the scab; yet he was successful. He concludes, that when the local affection is *regular*, matter for inoculation may be obtained from the fourth to the twelfth day. The knowledge of this fact may be useful in remote situations, though inoculation made with matter as soon as it can be obtained is most certain. Since this was written, I have learnt from a medical friend in London, from whom farther information on this topic may be expected, that matter taken from a vaccine pustule on the fourteenth or fifteenth day did not produce the Cow-pox, but severe ulceration. As the time is not strictly known when the matter assumes this change, it seems right, for the present, to restrict the taking of matter to that period, during which it has been found uniformly mild and efficient. This corresponds, in general, with the fifth, sixth, seventh, and eighth days after inoculation.

From what I have seen, I must conclude that the period at which the inflammation begins, does not serve invariably for discrimination between the regular Cow-pox and the irregular effect of inoculation, nor does the time the vesicle continues. The case referred to shows that its duration alone ought not to be relied on. But no case is recorded of the fluid having disappeared within two days from a vesicle that had the true character of Cow-pox: the vesicle of the shortest duration I have seen lasted many days.

The character of Cow-pox can be learnt only by the eye, yet some difference between the true disease and the spurious effects of inoculation admits of description.—The vesicle of the true Cow-pox has, in its early period, a milky whiteness: the fluid it contains is almost colourless, is as clear as water, and

and does not change into pus :* it hardens into a cruſt, or the veſicle burſting, it is diſcharged. Redneſs appears around the inoculated part in the ſpurious caſes, but the effloreſcence of true Cow-pox is more yellow or ſcarlet;† and ſeems to the naked eye to lie upon the ſurface of the ſkin, in appearance not much unlike the eruption of meaſles or ſcarlet fever. In the ſpurious caſes, the redneſs is leſs inclined to yellow, is ſometimes purpliſh, and ſeems to be under the ſurface of the ſkin. In all effective inoculations there is a deep hardneſs around the veſicle, that in general begins before the effloreſcence, or at the time of its appearance. A ſimilar hardneſs may be felt in the latter period of ſome irregular caſes.

The accurate attention to the riſe of the veſicle, and the appearance of the effloreſcence around it, cannot be too ſtrongly enforced; for it is only on the regular appearance of the inoculation in theſe ſtages that confidence ſhould be placed for ſecurity against the Small-pox. Should future experience diſcover that ſome irregular effects of inoculation afford ſecurity againſt the Small-pox, yet this injunction is not too ſtrict for the preſent.

If the diſeaſe commence, as is moſt uſual, on the third day, and loſe the character of Cow-pox within ſix days from the time of inoculation, I think it ſhould not be relied on; for in ſome inſtances Dr. Woodville inſerted matter of Small-pox on the fifth day after inoculation for the Cow-pox, and each diſeaſe had its courſe. The trial has not yet been made on the ſixth, ſeventh, or eighth days; but Mr. Loy, of Pickering, informs me, that on the ninth, tenth, and eleventh days he found the inoculation with Small-pox matter abortive. If the fact always prove ſo, it is probable that the conſtitution is ſecure on the ninth day, or it becomes ſecure after this period, within the time that the variolous matter requires for commencing its action. If the Cow-pox loſes the true character within the firſt nine days, it would, I think, be proper to ſubject the patient to a ſecond inoculation, to aſcertain his ſecurity againſt the Small-pox.

From the reſult of Dr. Woodville's experiments, and the well-known fact that the inoculated Small-pox commence about the eighth day after inciſion, it is evident, that the inoculation of Cow-pox is not a ſecurity againſt the Small-pox, when it anticipates the period of commencement of that diſeaſe only thirteen

* Sometimes at a late period, the margin of the cruſt ſeparates, and a purulent fluid oozes out; but this does not frequently happen.

† The colour is a leſs brilliant ſcarlet in cold than in temperate weather. In cold weather it is proper to clothe the inoculated arm rather warmer than uſual.

thirteen days; and therefore, as it is well ascertained that the infection of the Small-pox received casually produces the disease about the twelfth day, the inoculation of the Cow-pox ought not to be relied on for security, in any instance, in which a person has been previously exposed to the effluvia of Small-pox. If the ninth day after inoculation be the earliest period at which the body becomes secure through the Cow-pox, then, to give security, the Cow-pox must anticipate the period of commencement of the Small-pox by seventeen days: And a person inoculated with Cow-pox will be liable to injury from effluvia of Small-pox for five days. If similar trials be made on the sixth, seventh, and eighth days, it may be determined exactly how long after inoculation for the Cow-pox, a person may be injured by exposure to the Small-pox.* But as the Small-pox sometimes commence a little earlier, or a little later than the period mentioned, this time may occasionally vary. These observations are stated merely as suggesting rules useful for the present: future experience will probably shew that they need correction.

Irregularities in the course of inoculation are not observed in the Cow-pox alone; instances have sometimes occurred of irregular effects from inoculation for the Small-pox, which were erroneously supposed to afford security against future infection. I transcribe the following as they are related by a medical practitioner of great accuracy and high reputation:—"Last Spring (1772) I inoculated two children in one family; on the third day there was a slight inflammation around the places of incision; on the fifth it was considerably increased, and the places felt hard upon being pressed by the finger. I saw them again on the seventh or eighth day, and then the inflammation was much increased, extending nearly to the breadth of half-a-crown. Upon my applying a gentle pressure to the inoculated places, matter issued out of them; with which, as it issued from the arms of both patients, I perfectly saturated a cotton thread. With this thread I inoculated nineteen persons; every one of them had a fever, and eruptions of pustules at a proper time. But the children, from whom the matter was taken, did not sicken as was expected; and, on the eleventh day, the inflammation upon their arms was considerably abated; and two or three days after this, there remained nothing but a dry scab. Agreeably to the general opinion of the faculty, I told the parents, that their children were secure from future infection of

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* If these speculations may be relied on, do they not offer the strongest motive to early inoculation for the Cow-pox, since we cannot fly to that operation for safety when the hour of danger is arrived?

of the Small-pox. They however insisted upon their being inoculated again, which was accordingly done in the arm of each.—Contrary to my expectation, their arms began again to be inflamed, and went on in the same manner as they had done before, till about the ninth or tenth day; when they sickened, had a smart fever for three days, and then an eruption of a considerable number of variolous pustules."*

If it will not intrude too far on your paper, I request you to add the declaration of many eminent Physicians and Surgeons in London, published last August, as it may be unknown to some of your Readers. It is a very ample testimony to the truth of the facts which have been laid before the public in your paper. If you cannot with convenience insert it this week, I recommend it to your attention at some future opportunity. I am,

SIR,

Your obedient servant,

York, Nov. 26, 1800. ROBERT CAPPE.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

IN the following observations on a letter from Mr. Franks, inserted in the last Number of your Journal, I hope I do not improperly or unhandsomely anticipate his promised communication.

As Mr. Franks says that, in speaking of the inoculated Cow-pox and Small-pox, I have *magnified the character of one at the expence of the other*, I have carefully read again what I have written, and am, in conscience, obliged fairly to acquit myself of this charge. But I will not assume the right of a jury; permit me to lay the facts before your readers, not indeed to justify myself, for it is probable none will care whether I am right or wrong, but because Mr. Franks, without stating the premises, has hurried to a conclusion, contradictory to the general belief of the whole medical profession, and opposed to the opinion of some eminent members of that body, who through

* Mr. Dawson's Paper in Transactions of College of Physicians in London, vol. 3. p. 385. See eight similar cases related by Mr. Kite in the Memoirs of the Medical Society, London.

through a series of years, have made the contagion of Small-pox the subject of continued observation and enquiry.

Mr. Franks admits that the Vaccine Inoculation introduces a disease milder than the inoculated Small-pox; and he admits that the Cow-pox are not contagious: In this we are agreed. Mr. Franks himself acquits me of having exaggerated the merits of the Cow-pox; and most of your readers, I think, will acquit me of the charge of detracting from the character of the inoculated Small-pox; for, in the paragraph quoted by Mr. Franks, I have expressed myself much less strongly than Dr. Haygarth in his Sketch of a Plan to exterminate the Small-Pox; and his opinion cannot be supposed to turn to any unfavourable bias, since his purpose was to recommend inoculation to check the spreading of contagion. Having lately read his works with uncommon interest, I have unconsciously fallen into expressions not much unlike his; the following are his words: "As far as my circle of observation extends, both in England and Wales, this improved method of communicating the distemper has manifestly appeared to be injurious to the poor, though eminently useful to the rich. It has become prejudicial to the community, though human art never bestowed so valuable a blessing as it confers on the few intelligent individuals who adopt it. It has been falsely attributed by inattentive observers, to the inoculated patients spreading the casual infection, an event which, in a few instances, might inadvertently happen." In these words Dr. Haygarth acknowledges the fact implied in my letter; and also, his belief, that the inoculated Small-pox are infectious. In other parts of his works he has shewn, that the casual Small-pox owe their prevalence less to effluvia arising immediately from the patient, than to contagious matter adhering to clothes and other articles in common use. Now, it is well known, that the matter contained in the pustules that arise in the inoculated Small-pox does in no respect differ from the matter generated in the casual disease; it must, therefore, in proportion to the quantity, be equally liable to be distributed. In general, the pustules are not so numerous, and the quantity of infectious matter generated, is not so great in the inoculated as in the casual Small-pox: the chance, therefore, of the disease being conveyed from one person to another, is less; but it does not vanish into actual security. Yet Mr. Franks, without reserve and without proof, affirms it to be "a common prejudice, that the variolous inoculation gives origin to a contagious disease." And he denies that the Cow-pox possess any advantage over the Small-pox in this respect. This opinion not only contradicts general belief, and should therefore not be

be lightly received; but it cannot be eſtabliſhed by any number of inſtances in which the inoculated Small-pox have not ſpread infection, ſo long as any inſtance appears in which contagion can be traced to ſuch ſource. In examining this queſtion, it ſhould not be forgotten that Dr. Haygarth has adduced deciſive proof, that the contagion of the moſt malignant caſual Small-pox may be circumſcribed, and prevented from ſpreading even to the neareſt neighbour.

If any perſon doubt whether ſociety may derive advantage from the introduction of the Cow-pox, the doubt ſhould be openly and fairly ſtated, with the facts on which it is founded. If it be ill founded, it cannot be too quickly diſpelled; if it be well founded, it cannot be too ſoon or too generally known. But I muſt be excuſed in thinking it unjuſtifiable to publiſh, without proof, opinions that may lead any one to diſregard or undervalue the advantages that many cautious and experienced perſons expect from the introduction of the Cow-pox.

Mr. Franks is perhaps, in general, right in the opinion, that the air is not contaminated to an injurious degree by inoculation; that probably happens only when a confluent eruption or numerous puſtules are produced. Yet there are many other ways in which contagion may be conveyed from the ſick to thoſe who have not had the diſeaſe. Theſe facts ſuggeſted, and I am ſorry they juſtify, the expreſſions in the paragraph to which Mr. Franks objects.

If Mr. Franks's opinion be erroneous, he leads us to imagine our friends ſecure while ſurrounded by danger. A vigilant caution ſhould ever preſide over ſpeculations in affairs that nearly concern the lives of our fellow-creatures. I am,

Gentlemen,
With great reſpect,
Your obedient ſervant,
ROBERT CAPPE.

York, Dec. 11, 1800.

Experiments and Obſervations relative to Medicinal Barks.
By Dr. W. G. Maton, F. R. S. &c.

If it be admitted that the properties of ſubſtances differ according to the different proportions of their ingredients (and it would ſeem that this principle muſt be admitted in medical as well as in every other natural ſcience), nothing can be more evident than that thoſe moſt allied in compoſition muſt approach moſt nearly to each other with regard to their effects. By purſuing this compariſon alſo we are moſt likely to ſucceed in aſcertaining

certaining on what ſpecific ingredient, or combination of ingredients, the efficacy of a particular ſubſtance depends; an enquiry which has a powerful claim to the attention of medical profeſſors, but in which, perhaps, leſs progreſs has been made than in any other connected with the healing art. Unaided indeed by the important facts revealed by modern chemiſtry, ſpeculations of this nature *could* not be very ſatisfactory; but even now that the ſtudy of phyſic has received a conſiderable ſhare of the benefits which this ſcience has conferred on the uſeful arts in general, it has ſcarcely begun to emerge from the diſgrace and obſcurity of empiriciſm.

The above remarks will more particularly apply to ſome of thoſe vegetable ſubſtances which are conceived to poſſeſs what is called a *tonic* property. In this diviſion of the materia medica, the Peruvian bark deſervedly holds a diſtinguiſhed place; but whether there are not other ſubſtances (ſome of them indigenous in our iſland) which may in many caſes be exhibited with equal utility, is ſtill a queſtion with many eminent members of the medical profeſſion, and muſt continue to be ſo until chemical analyſis has diſcovered the preciſe compoſition of this celebrated bark, compared with that of other vegetable productions which contend for equal reputation. Some experiments connected with ſuch an enquiry, it is my object to bring forward in the preſent paper; but they form ſo inconſiderable a part of the ſeries neceſſary to the deciſion of the important queſtions above alluded to, that I bring them forward with much heſitation, and hope that they will be regarded rather as hints to perſons who have greater leiſure for the proſecution of theſe curious inveſtigations, than as an addition of much value to the ſtock of medical facts.

By no analyſis of the Peruvian bark hitherto publiſhed,* has it appeared that the principle denominated by the French chemiſts *tannin*, was diſcoverable in that ſubſtance; indeed, we do not any where find this principle referred to prior to the experiments of Seguin, which had in view the elucidation of the *rationale* of tanning, but which like many diſcoveries at firſt connected apparently with only one particular art, may hereafter throw a new light on other ſubjects of much higher importance.† As this point did not appear to have been touched upon,

* Since the above was written, a recent number of the *Annales de Chimie* has announced that Dr. Woſtring, a Phyſician of Norcop, in Sweden, has aſcertained the proportion of tannin contained in the ſeveral ſpecies of Cinchona. We are not informed, however, of the requiſite particulars relating to thoſe experiments.

† See "*Rapport au Comité de Salut Public, ſur les nouveaux moyens de tanner les cuirs, propoſés par le Cit. Armand Seguin.* 3. *Brumaire, l'an* 3 *de la Rep.*" (1795.)

upon, it was the firſt which I propoſed to myſelf to inveſtigate, and the following is the reſult of my experiments with the Peruvian, Caſcarilla, Oak and Elm barks, and Gall-nuts.* Half an ounce of each, very finely powdered, was infuſed two hours in half a pound of ſpring water, care being taken to ſtir the mixture very frequently; and at the end of this time, half an ounce of a ſolution of iſinglaſs (made by diſſolving half an ounce of the latter in one pound of water) was poured into the ſtrained infuſion. It was obſervable that the liquor ſtrained from the Peruvian bark was by far the cleareſt, and that of the Caſcarilla the moſt turbid. The elm bark contained ſo large a quantity of viſcid yellowiſh mucilage, that it was with the utmoſt difficulty the infuſion could be ſtrained, and it ſtained the paper, &c. through which it was filtered of a reddiſh brown colour: that left by the other barks was pretty eaſily waſhed off by means of pure water only, but the colour of the elm yielded only to the infuſion of galls, which effectually eradicated alſo the others. Another circumſtance proper to be mentioned was, that on a certain quantity of the infuſion of galls being accidentally poured into ſome of the infuſion of bark, the colouring matter of the latter was ſpeedily precipitated, and formed a pinkiſh cloud at the bottom of the veſſel: a ſimilar effect was produced on the infuſion of Caſcarilla, but not on the other infuſions.—No change being produced on half an ounce of the liquor of the Peruvian bark by half an ounce of the ſolution of iſinglaſs, the quantity of the former was doubled; in a few minutes a very ſlight flocculent appearance was perceivable, which however, during half an hour, did not form more than a cloud. Another half ounce of the infuſion was added, but at the end of forty hours the precipitate was ſcanty, and weighed only a quarter of a grain. When dry, this precipitate became tough, ſomewhat tranſparent, and of the colour of carmine.—2. One ounce of the infuſion of Caſcarilla was not affected by the ſolution of iſinglaſs; but ſoon after another half ounce had been added thin flocculi collected pretty faſt, yet it was not until forty hours had elapſed that the precipitate could be collected, and it was then pretty compact, brown, and weighed 1¾ grain.—3. Equal quantities (half an ounce) of the infuſion of oak and ſolution of iſinglaſs yielded a precipitate immediately, the mixture having firſt aſſumed a curdled appear-

* Great pains were taken to procure the beſt kinds of theſe ſeveral ſubſtances; the Peruvian Bark which I employed was the red ſort, and obtained, as well as the Caſcarilla, at Apothecaries Hall.

ance, which was found to consist of an infinite number of fine flocculi coagulating quickly, and in a most beautiful manner, into a dark brown sediment. I could not doubt, that, at least with respect to this bark, I had obtained the neutralized tannin of Seguin, whose description of the precipitate which he had procured from the same bark, corresponded exactly with mine. The precipitate did not adhere to the fingers even when moist, but was readily squeezed into a compact mass, which became extremely hard and brittle, and of a blackish brown colour; it weighed, when quite dry, rather more than four grains. Half a drachm of solution of isinglass was added to the liquor poured off from the precipitate, but there was no appearance of flocculi; the isinglass, at the end of forty hours, had sunk to the bottom of the vessel almost pure. — 4. The strained infusion of the elm bark exhibited a slight flocculency as soon as the liquid isinglass was added. After the space of forty hours, a considerable precipitate was formed, which, when dried, weighed six grains, but was not very compact; the colour reddish brown.—5. The infusion of galls, after having been treated similarly to that of the oak bark, changed instantly from a light greenish brown to a milky white colour: the mixture was now nearly tasteless. At length a white precipitate was formed, which, when weighed, was found to amount to the quantity of eleven grains and a half, and had become a very hard but friable mass. To the liquor poured from this precipitate I added half an ounce more of the solution of isinglass. The mixture assumed the appearance of whey, and yielded another precipitate, not so strongly formed as the first, but weighing five grains and a half, and its colour was more inclined to yellow. Half a drachm of solution being added to this last liquor, and the whole quantity allowed to stand forty hours, nothing but isinglass was to be seen at the bottom of the vessel, and the liquor had begun to be more of a green colour, which gradually deepened towards the surface. The last-mentioned circumstance was observable with respect also to the Peruvian and the Elm bark infusions (strained from the precipitates) after they had stood the same length of time.

It was now necessary to make an accurate examination of the above-mentioned precipitates, and to ascertain whether they were all really the combination of the gelatine with the tanning principle of the respective barks. But I was desirous of discovering previously, the relations of the several infusions to one another, with respect to colour, after an admixture of each with a certain quantity of a solution of iron; and of comparing the appearance of the infusions which contained all the ingredients

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in ſolution, with that of the liquor from which the precipitates had been obtained; for I was not convinced that a portion of gallic acid could not have gone down with the *tannin* in conſequence of being alſo neutralized by the gelatine. This indeed was but a rough mode of aſcertaining the portion of gallic acid, but it ſeemed to be worthy of trial. Half a drachm of a ſolution of the green ſulph. of iron in diſtilled water was added to half an ounce of each of the infuſions which had formed precipitates, and to a like quantity of the ſame not deprived of their *tannin*. The colours did not prove more intenſe in the laſt-mentioned infuſions than in the firſt. There was a ſtriking difference, however, in the quantity of gallic acid manifeſted by the ſeveral barks, the Oak aſſuming a deep inky purple colour, as did alſo the Peruvian; whereas the Caſcarilla was but ſlightly altered, in compariſon with the others. The Elm infuſion acquired a browniſh purple tint, which was pretty deep; the galls, a purple more inclined to red.

It was plain that all the barks had been infuſed long enough to evolve at leaſt a part of their gallic acid, and I was of opinion that the above experiment was not nice enough to determine whether any had gone down in the precipitates: this could be decided only by an accurate analyſis of the precipitates themſelves. That gallic acid has an affinity to animal gluten is ſufficiently evident from the memoir of Cit. Seguin, who thinks that it has its effect in the proceſs of tanning, though whether detrimental or otherwiſe he does not appear to have proved.*

Why a longer portion of time has been preſcribed for the infuſion of barks, in order to extract the gallic acid, than has been thought neceſſary for the extraction of tannin, was another point concerning which I was by no means ſatisfied; and I began to doubt whether by an infuſion of only two hours I had procured as much of the *latter* as the barks might really contain, eſpecially as ſome ſeemed to give it out to the water more ſlowly than others. It was likewiſe uncertain whether the proportion of the bark to the menſtruum was ſuch as was moſt favourable to the complete evolution of all the ſoluble parts of that ſubſtance. It appeared therefore proper to obſerve the effects of the ſolution of iſinglaſs on an infuſion of longer ſtanding than has been thought ſufficient in pharmaceutical proceſſes;† and alſo

* Mr. Biggin, who (though ſo well known before as a gentleman of talents and zeal in the proſecution of philoſophical experiments) juſtly deſerves diſtinguiſhed mention for his examination of ſubſtances moſt advantageous in tanning, has informed me that he is convinced of the *injuriousneſs* of this acid.

† See Lewis's *Materia Medica* (laſt edit. by Dr. Aikin), article *Cortex Peruvianus*.

alſo to try at what point of time, or by what means, the liquor might be known to hold all the *tannin* that the bark contained.

[To be continued.]

Craven Street, Nov. 20, 1800.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

I Lately tranſmitted ſome obſervations on Bronchocele, which you have done me the honour to inſert in the laſt Number of your very reſpectable Journal. I beg leave to correct an error in the formula recommended for the complaint. Inſtead of directing the maſs, containing two ounces of burnt ſponge, to be divided into forty-eight, as I ought to have done, I directed it to be divided into twenty-four. This inadvertence probably aroſe from my knowing the perfectly harmleſs nature of the remedy; and muſt be apparent to every reader, from my ſtating in another part of my paper, that every lozenge is to contain a ſcruple of the medicine.

This is the doſe I have hitherto given, and, except in one inſtance, only twice a day. I intend to increaſe the quantity to half a drachm, or more, in caſe the patient can bear it. The only difficulty ariſing from it is, that from the flavour, and its being held in the mouth till it diſſolves, nauſea is excited in ſome delicate habits; ſuch as thoſe commonly are who are liable to this diſeaſe.

I am more and more convinced that the treatment of this diſorder is not well underſtood, having been informed by different medical men, ſince the publication of my former paper on this ſubject, that in the courſe of their obſervation and practice, they had known but little benefit derived from the means employed for its cure.

From a gentleman of great eminence in the profeſſion I learn, that his friend, the late Dr. Bromfeild, died of this complaint; and that the tumour was opened about a week previous to his death, an operation which ſerved only to prolong a miſerable exiſtence.

In perſons rather advanced in years, the Bronchocele will not always yield to medicine; but its commencement frequently takes place early in life; and if due attention is paid to it in time, I have reaſon to believe that it will generally prove curable. Particular care ſhould be taken that the medicine is not left

left off too ſoon; for the diſorder is of an inſidious nature, and apt to deceive thoſe who are not much accuſtomed to it. I have ſeveral times known patients leave off their medicine in this caſe, from an idea that the Bronchocele was cured; and after a while, they have been obliged to have recourſe to it again. It is, however, poſſible that the diſpoſition only to the diſeaſe might remain, and cauſe the tumour to regenerate. Be this as it may, the effect of returning to the uſe of the lozenges has been the ſame; if properly perſevered in, they have ſeldom failed to exterminate the complaint.

To theſe remarks I beg leave to ſubjoin a caution to thoſe perſons who venture to give quack medicines. A phyſician lately mentioned a caſe at a medical ſociety, in the preſence of one, at leaſt, of the Editors of this Journal, which ought to ſerve as a warning to all ranks of perſons in this age of empiriciſm.

This caſe I ſhould not have preſumed to communicate to the world, had I reaſon to believe that gentleman himſelf would publiſh it; but he gave the ſociety reaſon to conclude he had no ſuch intention, by ſaying, that he was not much in the habit of publiſhing. This is to be regretted in a practitioner of eminence, and eſpecially in one who has ſingular opportunities of ſeeing uncommon caſes.

Whether this averſion to appear often in print ariſes from diffidence, or want of leiſure, I pretend not to determine; but as there are ſo many reſpectable channels open for the ready communication of uſeful caſes, I ſincerely wiſh it was in my power to prevail on other diſtinguiſhed members of the profeſſion to follow the example of a Denman; and not conſider it as any diſparagement of their dignity to publiſh a ſingle caſe, or a ſingle uſeful remark, in any vehicle of information which may be likely to fall into the hands of medical practitioners at large, and to prove beneficial to the world.

The caſe to which I now allude was one in which Dalby's Carminative, given in too large a doſe, proved fatal. The doſe, according to the nurſe's account, was a tea-ſpoonful; the age of the child about a twelvemonth. Upon examination after death, the injury appeared to have been done to the brain; a remarkable degree of congeſtion appeared in every ſinus, and every veſſel of the pia mater, ſo as to leave no room to doubt that the cauſe of death was ſeated there.

This, and other caſes, where exceſſive doſes of opium have proved deſtructive, as well as one publiſhed in your Journal by Mr. Brookes, where a fatal conſequence was prevented by venæſection, ſeem to confirm the propriety of that gentleman's practice;

practice; and to shew that unloading the vessels of the head is one of the chief indications of cure.

I have known Storey's Worm-cakes prove fatal; and, what made the case still more affecting, they were given by a father to a child, who had never shewn a single symptom of having worms.

In regard to Dalby's Carminative, it is necessary to remark, that the physician who mentioned the fatal case, also related an instance in which it proved very pernicious to an adult; who, having been used to take it for a complaint of the stomach or bowels, sent for a bottle to a different shop from that where she used to buy it, and it proved so much stronger than what she had been accustomed to take, that, with the able assistance of this experienced practitioner, she did not recover without much difficulty. I am,

GENTLEMEN,

Your most humble servant,

JOHN KING.

New Street, Hanover Square,
Nov. 11, 1800.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

SHOULD the annexed observations on a branch of Midwifery merit a place in the Medical and Physical Journal, they are at liberty for insertion in your next Number. I am,

GENTLEMEN,

Your humble servant,

JAMES BARLOW.

Blackburn, Lancashire,
Nov. 20, 1800.

On the Advantages and Disadvantages of inducing premature Labour, with a View of superseding Embryulcia, the Section of the Symphysis Pubis, and the Cesarean Operation.

Nec Deus intersit nisi dignus vindice nodus. HORACE.

THE practice of exciting premature labour, in cases where the pelvis is so much distorted as to render embryulcia necessary, has been recommended by various authors, though the exact limits prescribed for its necessity have not hitherto been pointed out with that degree of precision which the importance of the operation demands. How far I have been able to illustrate the subject I cannot pretend to say: if, however, I am so fortunate

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as to caſt a ſingle ray of light thereon, I ſhall not conſider my time miſpent.

To enter into a detail of all the various modes of delivery in caſes of deformity of the pelvis, would far exceed the limits preſcribed to the preſent undertaking; I will therefore only make a few animadverſions on the ſubject in queſtion, and wave entering into a laboured hiſtory of the operation, nor trouble my readers with aſcertaining the exact epoch which gave rise thereto. The various gradations of deformity incident to the female pelvis, are frequently ſuch as involve the accoucheur in ſome degree of perplexity reſpecting the beſt mode of delivery to be adopted for the ſafety of both mother and child, and various are the reſources which obſtetric knowledge indicates for that purpoſe. The cruelty inflicted upon the fœtus by the uſe of the crotchet, and the danger attendant upon the mother, are reaſons of ſufficient weight for the humane accoucheur to exerciſe his utmoſt ſkill in endeavouring to point out a more eligible expedient, in caſes of diſtorted pelvis, than that of the deſtruction of the fœtus. The deformity of the pelvis then, to a certain degree, is what has been conſidered by authors as an indication for bringing on premature labour, with a view of ſaving the life of the child, and leſſening the riſk of that of the mother.

On a ſuperficial view of the ſubject, it would appear to be pregnant with but few difficulties; but, on a more minute inquiry into the attendant circumſtances, we ſhall be compelled to acknowledge that many obſtacles preſent themſelves, ſome of which appear of conſiderable cogency: whether the removal of theſe lie within the ſphere of obſtetric art, or, otherwiſe, wait the interference of the legiſlature, time alone can determine; it is nevertheleſs incumbent on every accoucheur (before he attempts the operation) to eſtabliſh in his own mind the morality of the practice, and exert his utmoſt ſkill, by every poſſible means, to extricate the environed fœtus from impending deſtruction.

This operation, however, can ſeldom be put in practice before it has been proved, by the event of a former labour, that the pelvis was ſo much diſtorted, that the life of the child muſt have been inevitably deſtroyed before delivery could be accompliſhed. And as the human pelvis is liable by diſeaſe to become contracted into various ſhapes and degrees of diſtortion, from that of a well-formed pelvis to one ſo much contracted in its different apertures, that delivery per vias naturales becomes in ſome inſtances utterly impracticable; in theſe different ſhades of diſtortion, art has furniſhed us with means of accompliſhing delivery in various ways, according to the exigency

gency of each individual deviation from the natural ſtandard: but as authors, even in the preſent improved ſtate of the art, differ in opinion on theſe important points of practice, it is to be hoped that every laudable endeavour to facilitate the difficulties attendant on parturition, will promote a free inquiry on the ſubject, and ultimately have a tendency to the general good of ſociety. To become acquainted with the fundamental principles of midwifery, and to aſcertain the exact dimenſions of the pelvis, is a matter of the greateſt moment to the accoucheur, before he determines on the operation; as want of this neceſſary knowledge made by the touch, may involve the practitioner in irretrievable injury to the fœtus. The diſtortions of the pelvis are generally induced by *rachitis* in the infantile ſtate, or by *malacoſteon* and *exoſtoſis* in the adult: it is theſe different cauſes and degrees of diſtortion which the accoucheur ſhould keep in view when making the admeaſurement per vaginam. When paſſing the finger up the vagina, if any part of the os ſacrum, or lumbar vertebræ be perceived to project into the cavity of the pelvis, or if the rami of the iſchia, or the ſymphyſis of the os pubis, approach unuſually near each other, we may conclude that ſome degree of original mal-conformation exiſts in the bones which compoſe that aperture; yet it not unfrequently happens that the ſuperior aperture is conſiderably contracted, and the inferior cavity, or outlet, is even wider than natural: and to obtain a ſufficient idea of the ſtate of the apertures of the pelvis, in every direction, I have frequently found it neceſſary to have recourſe to the paſſing of the whole hand into the cavity of the pelvis; the three firſt fingers of which are to be conducted to the brim, where the admeaſurement may be generally aſcertained to the ſpace of a few lines, by placing them in different directions in the ſuperior ſtrait; and if, when kept cloſe together, the ſide of the fore-finger touch the os pubis, and the third the projecting angle of the ſacrum, we may conclude that the ſpace betwixt the two points is manifeſtly no more than two inches, an opening through which no living mature fœtus can poſſibly be extracted alive, and when only two fingers can be placed in the ſame manner, and betwixt the two angles above-mentioned, the accoucheur may reſt ſatisfied, that, except a greater ſpace can be obtained on either of the ſides where the admeaſurement is made, no other mode of delivery, per vias naturales, can, with ſafety to the mother, be adopted, than either the crotchet or Cæſarean ſection. This method of proceeding will generally furniſh the accoucheur with an accurate knowledge of the dimenſions of the pelvis, and direct him to the moſt eligible reſources in caſes of extreme deformity.

mity. In every inſtance where I have had occaſion to terminate the delivery with the crotchet, it has been my conſtant practice to aſcertain the exact dimenſions in every poſſible direction, that I might be armed with every requiſite auxiliary in each ſucceeding labour.

To thoſe whoſe opportunities of practice have not furniſhed them with a ſufficient ſhare of diſcrimination by the touch per vaginam, to be enabled to aſcertain to a tolerable degree of exactity the various ſhades of diſtortion which the female pelvis is liable to. I would recommend the uſe of the *calipers*, as an inſtrument more eaſily applied, and poſſeſſing ſuperior advantages of accuracy over any other. See plate the 6th of Heath's Baudelocque; and for an account of their mode of application and utility, I will tranſcribe the 130th page from Vol. I. of that author.

"To determine how much the ſuperior ſtrait is defective in the aforeſaid diameters, and meaſure it by means of theſe compaſſes, we take the thickneſs of the woman, from the middle of the mons Veneris to the centre of the depreſſion of the baſe of the ſacrum poſteriorly, by applying one of the points of the inſtrument before, againſt the ſymphyſis of the pubis; and the other behind, a little under the ſpine of the laſt lumbar vertebra; and deduct three inches from that thickneſs in women that are thin, for the baſe of the ſacrum, and the anterior extremities of the oſſa pubis; the thickneſs of theſe latter being at moſt but ſix lines, and that of the baſe of the ſacrum two inches and a half; and ſo conſtantly ſo, that I have not found a difference of a line in about five and thirty pelves, diſtorted and contracted in all manner of ways, and in all poſſible degrees.

"This ſubtraction of three inches from the external thickneſs of the pelvis, in the ſaid directions, is alſo ſufficient when the luſtineſs is moderate; and we may add one or two lines more, when it is exceſſive, becauſe the fat which forms the mons Veneris, eaſily ſhrinks under the lenticular extremity of the leg of the compaſſes. The reſult of this procedure is ſo exact, that the pelvis meaſured with the common compaſſes, after opening the body, was not above a line over or under my eſtimation in any one of my experiments. A greater preciſion, if we could obtain it, would be uſeleſs, ſince the choice of the moſt proper methods for terminating the delivery in a given caſe, cannot depend on a line more or leſs in the diameter of the pelvis. According to theſe data, the knowledge of this diameter is eaſily obtained. It is four inches when the external thickneſs of the pelvis meaſures ſeven; it is but three when the latter only meaſures ſix; and but two when it does

not exceed five, &c. I ſuppoſe the woman to be thin, as moſt of thoſe are who have been ricketty."

In conſidering the mechanical deſcent of the fœtal cranium through the pelvis, it will appear manifeſtly neceſſary that the dimenſions of the one ſhould bear ſome mathematical proportion to the other. To obviate certain difficulties, Nature has wiſely ordained a peculiarity of ſtructure to the fœtal cranium, the mechaniſm of which is ſo formed, that the head of the fœtus is leſs compact than that of the adult, and the bones more looſe and numerous, conſequently are better adapted for paſſing through the pelvis, than if they had been firmly joined together.

The growth of the fœtus alſo varies very much during the different ſtages of geſtation. The embryo increaſes more rapidly in ſize during the firſt weeks after conception, (than after it takes on the fœtal ſtate, till the end of nine months,) though this increaſe has various ſtages of irregularity, till the full period of utero-geſtation. The walls of the pelvis being ſo firmly connected together, and the change which the fœtal head is conſtantly undergoing as geſtation advances, will ever remain an obſtacle to this ſpecies of delivery, owing to the uncertain magnitude and degree of reſiſtance oppoſed to the pelvis during its paſſage through that tube. Without multiplying the various poſitions which the fœtal head aſſumes during the approach of labour, it may be neceſſary to ſtate, that the general and moſt natural preſentation, is the vertex with the ears nearly in a diagonal direction betwixt the pubis and ſacrum. The cauſe of this part of the head deſcending firſt into the pelvis, may be owing in ſome meaſure to the foramen magnum being ſituated nearer the occiput than the face, conſequently is more mechanically inclined to become firſt puſhed into the ſuperior aperture of the pelvis; ſometimes, however, the anterior fontenelle may at the commencement of labour be perceived to preſent in the axis of the ſuperior ſtrait; but this ſmall deviation is generally rectified by the repeated action of the uterus, which forces the vertex into the axis of the pelvis, whilſt the chin is bent down upon the breaſt, and the face turned into the hollow of the ſacrum. If this proceſs was not to take place, and the fontenelle was forced out of its proper direction, the labour would prove tedious, and probably might ſometimes terminate in a face preſentation. The forceps and lever are inſtruments well calculated for extracting the fœtus within certain bounds of difficulty, and claim a preference over every other invention, when uſed with a view of preſerving the lives of both mother and fœtus; and when the ſmall diameter of the ſuperior ſtrait of the pelvis offers a

ſpace

ſpace for the entrance of the head of the fœtus, equal to three inches in diameter, we may not altogether reject every hope of extracting a living mature fœtus through an aperture of the above ſpecified dimenſions, with either of theſe inſtruments; or if the child's head is ſmall, and not too firmly oſſified to elongate, and allow the bones to overlap by preſſure, and the forcible action of the uterus, we may ſometimes under theſe circumſtances meet with a favourable termination, when the diameter of the pelvis is rather under three inches. On the contrary, when the mother's pelvis is aſcertained to meaſure no more from pubis to ſacrum, or in any other of the apertures, than two inches, or two inches and a half, I am perſuaded that no mature fœtus can poſſibly be brought through a ſpace of theſe dimenſions, without deſtruction to the child; it is then in this intermediate degree of diſtortion, betwixt the poſſibility of delivery (without injury to the mother or fœtus) with the forceps or lever, and that ſhade of deformity which requires the application of the crotchet, where premature delivery ſeems moſt likely to become admiſſible. A queſtion then will naturally preſent itſelf to every accoucheur who deliberates on the ſubject, namely, what degree of compreſſibility is the mature fœtal cranium capable of undergoing, without deſtruction to the child during its paſſage through the pelvis, and what analogy does it bear in computation with one of ſeven or eight months? The ſolidity of the fœtal head, and the re-action of the bones of the pelvis upon that body, are ſo variable in different children, that no exact criterion can be formed before birth; however, I will venture to riſk an opinion, that the diameter of the fœtal head, under certain ſituations, will bear a reduction in ſize by preſſure, from a quarter to half an inch, without producing much injury to the fœtus; but when the diameter of the head becomes much reduced beyond this, by either the forceps or lever, their application becomes dangerous to the child, and of courſe inadmiſſible on that ſcore. I conſider the fœtus a mere paſſive body during its paſſage through the pelvis; nevertheleſs, I am well aware that examples are not wanting, where the fœtal head has undergone a much greater reduction by compreſſure without proving fatal; however, I would have it underſtood, that what I have advanced, is what more generally takes place, and the deviations from the above rule are only exceptions therefrom. Some difference may reſult from the reduction of the volume of the cranium of a fœtus of eight, and one of nine months, as it will be generally allowed, that the bones which compoſe the cranium of the latter, will not admit of the ſame degree of reduction when compared to the former; yet, perhaps, a leſs

a lefs variation in the fize of the head will take place betwixt the two, during their paffage through the pelvis, than what at firft would appear probable, owing to the greater power of action of the uterus during labour upon the body of a fœtus of nine months, than upon an immature one of feven or eight; and fome difference of refult may alfo arife to the fœtus from the length of time which may elapfe during the paffage of the head through the aperture of the pelvis.

The next queftion which occurs is, in what degree of diftortion of the pelvis it becomes neceffary to have recourfe to this operation? This point will not be eafily afcertained, even if the accoucheur were allowed to make an examination per vaginam; and as this is a matter not altogether attainable during geftation, it becomes requifite to ftate the circumfcribed dimenfions of the pelvis, wherein this operation is more likely to meet with fuccefs. I prefume then that a pelvis, the fmall diameter of which meafures from pubis to facrum about two inches or two inches and a half, appears to favour the fuccefs of this operation more than any other dimenfions; for, on the one hand, it is fufficiently evident that a mature fœtus cannot be born alive, when the dimenfions are under two inches and a half; and on the other, when the fhort diameter does not exceed two inches, the crotchet becomes neceffary; and fhould the fuperior aperture meafure only one inch and a quarter in the wideft part of its fuperior conjugate diameter, the only refource for the fafety of the mother and fœtus is the Cæfarean operation.

If it is proved that this operation cannot be performed with any degree of certainty of fuccefs to the fœtus, except where there is a fpace to be gained in fome part of the fuperior aperture, from two inches to two inches and a half, then it will follow that this mode of delivery muft be very much limited, infomuch, that a confiderable degree of hazard will always attend it, owing to the want of accuracy in difcriminating betwixt the relative fize of the fœtal head and the dimenfions of the pelvis. Having now pointed out the diameter of half an inch, as the moft warrantable fpace allotted for the fuccefs of this fpecies of operation, when performed at any time from the latter end of the feventh to that of the eighth month of utero-geftation, before or after thofe periods I think no one juftifiable who induces premature labour under thefe circumftances; nor can I altogether give credit to thofe accounts, from whatever fource they may have been gleaned, where it is afferted that premature delivery has been fuccefsfully adopted when performed early in the feventh month of pregnancy, and where

where the diameter of the pelvis meafured not more than two inches in the wideft part.

When this operation is had recourfe to, and the dimenfions of the pelvis are fuch as promife fuccefs, we ought to defer the attempt as near to that period fixed by Nature for the full evolution of the fœtus as circumftances will admit, that thereby the child may acquire every poffible advantage to enfure a healthy ftate of exiftence after birth. The period of eight months, or thereabouts, is the moft advantageous time for the performance of this operation, when we confider the precarious ftate of a fœtus at a much earlier period; for I will venture to predict, that not one in twenty can furvive the birth fo completely as to be reared, where labour is excited earlier than the feventh month of geftation: no doubt, a few exceptions may be oppofed to this general conclufion; but if we confider the puny ftate of thefe fhort-lived individuals, and the mifery which their premature birth fubjects them to, we fhall have no caufe to envy their fituation; and few would prefer fuch an ephemeral ftate to a mere nonentity.

It is to be wifhed, that authors who have had recourfe to this operation had been more explicit in their accounts refpecting the exact period of geftation, and the premature fate of children, and how long they furvived that event; for it is not a fufficient incitement to the operation, merely to bring a being into exiftence which muft inevitably perifh foon after birth. And this immature mode of delivery would confequently be a means of fubjecting the mother oftener to a ftate of geftation than if fhe were allowed to complete the period of nine months. To obtain premature delivery, the membranes which envelope the fœtus are generally artificially ruptured prior to that event; and the time which elapfes before the uterus refumes its expulfive efforts, is very variable, infomuch that in fome cafes even feveral days have paffed before that organ has completed its evolution. When the waters are thus evacuated, and the fœtus left in clofe contact with that vifcus during its repeated contractions, I am difpofed to confider the life of the child, during its exit through the pelvis, involved in fome danger, and that proportionable to the degree of diftortion and date of geftation. It is not fufficient that premature labour fhould be indifcriminately produced at a given period of geftation, merely with a view of fuperfeding embryulcia; it is of more importance to look forward to the prefervation of the child; for every attempt made as early as the fixth month of geftation, either with the intent of avoiding embryulcia, or preferving the life of the fœtus, will anfwer no other purpofe than jugulare mortuos; for, as I have before obferved, and what I wifh further

to

to inculcate is, that every delivery excited by art, or otherwiſe caſually taking place at an earlier period of pregnancy than the ſeventh month, will generally prove deſtructive to the fœtus, either during the time of delivery, or very ſoon after that event.

The ſufficiency of augmentation of ſpace gained by the ſection of the ſymphyſis pubis in extreme deformity of the pelvis, and the manifeſt danger attending that operation, are circumſtances ſufficiently warrantable to preclude its uſe as a ſubſtitute for any of the above-mentioned modes of delivery. The Sigaultian operation frequently involves the fœtus in imminent danger when the diſtortion is conſiderable; and in ſome inſtances where the ſection has been made, the conſolidation of the ſacrum and ilia has been ſo firmly united, that no adequate ſpace could be gained by a diviſion of the ſymphyſis pubis for the extraction of the child, and delivery has afterwards been obliged to be terminated by the crotchet or Cæſarean operation; and it has been doubted, and with ſufficient reaſon, whether the Sigaultian operation has ever been ſucceſsful to either mother or child, in caſes where the ſuperior apertures from pubis to ſacrum, meaſured leſs than two inches and a half: hence will appear the inſufficiency of this operation, and the neceſſity of its being baniſhed in future from the practice of midwifery.*

It is a matter of ſome conſequence for the accoucheur to diſcriminate, in caſes of diſtortion of the pelvis, between exoſtoſis and malacoſteon, as in the latter the bones will ſometimes give way conſiderably, either by the introduction of the hand, or the impulſe of extracting the child.

Two caſes of this ſpecies of deformity have fallen under my care; in one of which, the projection of the lumbar vertebra and the connecting angle of the ſacrum were ſo much bent into the cavity of the pelvis, that on the introduction of the finger up the vagina, a protuberance preſented to the touch, very much reſembling the head of the fœtus, pretty far advanced into the pelvis; on carrying the finger a little higher up, paſt the projection, I could aſcertain the head of the child; but on moving the finger round the projecting part, the

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* Amongſt the numerous evils attending the Sigaultian operation, the laceration or ſeparation of the ſacro-iliac ſymphyſis is one of the moſt dangerous; to obviate which, and at the ſame time allow a ſufficient ſeparation of the ſymphyſis pubis, appears no eaſy matter; yet, to accompliſh which, I have however thought, that if a vice could be fixed externally upon the hips of the woman, with a ſcrew ſo managed, that during the operation and ſubſequent treatment, no more ſpace ſhould be allowed to take place than what was ſufficient for the extraction of the fœtus, the injury in queſtion would be in ſome meaſure obviated.

the diſtortion was ſo great, that the whole circumference, in any direction, did not exceed that of half-a-crown. This was on the 29th of April, 1792, at which time I delivered her with the crotchet, and the bones of the pelvis yielded conſiderably to the impulſe of the head of the fœtus, which acted like a wedge during the efforts of extraction; yet, notwithſtanding the pliability of the bones of the pelvis, and the debilitated ſtate of her conſtitution, ſhe recovered ſpeedily, and without interruption. On the 22d of February, 1794, I was in the neighbourhood where this poor woman reſided, and hearing of her being ſtill alive, I was led by curioſity to pay her a viſit, and, if poſſible, to obtain permiſſion to examine her per vaginam, which was readily granted. I found the rami of the oſſa iſchia at their junction with the rami of the oſſa pubis overlapped each other, leaving a ſmall opening under the ſymphyſis pubis, ſufficient to admit the finger to paſs into the vagina by that paſſage, and another aperture below, but rather larger, and parallel with the tuberoſities of the oſſa iſchia. From what I have been ſince able to learn, ſhe ſurvived this period near two years, at which time ſhe was become ſo crooked, that her breaſt and knees were almoſt in contact with each other, and after her death it was with difficulty ſhe could be put into the coffin. This woman bore nine children, and died in the 39th year of her age: I attended her in the preceding labour to that abovementioned, which caſe was lingering and tedious; however, I ſucceeded by terminating the delivery with the lever, and preſerved the life of the child. The other caſe of malacoſteon occurred to me in this town, about three years ago: the woman was in a dying ſtate before I was conſulted. On examination I found the pelvis very much diſtorted, and ſuſpecting the deformity to ariſe from a ſtate of malacoſteon, I was induced, from the circumſtances of the preceding caſe, to attempt delivery; and this more particularly, as the poor woman expreſſed an earneſt deſire to be releaſed from her preſent burden, rather than die undelivered. On inquiry, I learned that ſhe had only juſt completed the ſeventh month of geſtation; this ſtage of prematurity induced me to attempt to turn the fœtus, and deliver by the feet, by which means I might give the child a greater chance of life; but with reſpect to the mother, there was not the leaſt proſpect of her ſurviving delivery long. The os uteri being dilated, I accordingly introduced my hand, but the apertures of the pelvis were ſo much diminiſhed in their dimenſions, that it was with conſiderable difficulty; however, I ſucceeded, and delivered the woman of living twins; ſhe died in about twelve hours after, and the children did not ſurvive their birth many days. Having obtained leave to

to inſpect the body afterwards, I found the dimenſions and ſtructure of the pelvis as follow: The conjugate, or antero-poſterior diameter, from the ſymphyſis pubis to the projecting angle of the ſacrum, meaſured two inches and a half; and the antero-poſterior dimenſions on each ſide of the ſacro-iliac ſymphyſis, between theſe two points, were, in ſome places, a few lines more than two inches and a half; from which it is obvious, that the form of the ſuperior aperture of this pelvis had a triangular appearance. The rami iſchii approached ſo near each other, that the ſpace left betwixt them would ſcarcely admit one finger; and on the introduction of the hand, when proceeding to turn the fœtuſes, theſe bones receded from each other very conſiderably; and the ſame effects were obſervable in the ſuperior ſtrait. The ſpongy nature of the bones of this pelvis were alſo manifeſted, by having very little cretaceous matter in their ſubſtance, and their texture was ſo ſoft, that they were eaſily cut with a knife. Many advantages, I am confident, may reſult from an accurate knowledge of the diſeaſes and ſtructure of the bones of the pelvis prior to delivery: it is, I am perſuaded, from a want of this information, that the Cæſarean operation has ſo often proved fatal in this country, for there is reaſon to ſuppoſe, that moſt of the women on whom that operation has been performed in this kingdom,* have been afflicted with malacoſteon, and conſequently in an irrecoverable ſtate, independent of any injury inflicted by the operation itſelf; and perhaps, if the nature of the diſeaſe had been more clearly underſtood, delivery per vias naturales might, in ſome inſtances, have been accompliſhed with ſafety to the child, without having recourſe to any other mode of delivery. And in theſe caſes, where malacoſteon has made ſuch dreadful ravages in the female conſtitution, beyond which the art of medicine has hitherto fallen ſhort of re-eſtabliſhing, it would be a nugatory practice, when attempted only with the view of preſerving the life of the mother, to have recourſe to either the Sigaultian operation or the Cæſarean ſection, as the woman would inevitably die, even if exempt from a ſtate of geſtation. If we advert to the numerous caſes of Cæſarean ſection performed on the continent, compared with thoſe of this country, it will be obvious, that the comparative fatality has been owing to this diſeaſe pre-exiſting to the operation; for, in no inſtance where the Cæſarean operation has been performed in this country, has it proved ſucceſsful to the woman, except the caſe of Jane Foſter, on whom† I performed the Cæſarean operation

* England, Ireland, and Scotland.

† Vide Medical Records and Reſearches, p. 154.

operation with ſucceſs, at Blackrod, in this county, a few years ſince; in which caſe the operation became neceſſary, on account of a deformity of the pelvis, incurred by an accident prior to a ſtate of geſtation, and not from any conſtitutional diſeaſe. The failure of ſucceſs attendant on the Cæſarean operation betwixt this and a neighbouring nation, is more aſcribable to conſtitutional diſeaſe, which thoſe women had laboured under in this country, who have undergone the operation, than to any difference of climate or mode of operating.

When premature delivery is attempted, with a view of ſuperſeding the Cæſarean ſection, it can only become neceſſary when the dimenſions of the pelvis are ſuch as preclude every other mode of delivery per vias naturales: upon this ground the practice will be confined to the embryon ſtate, or at leaſt, ſoon after it has arrived to that of the fœtal period. The ſolicitude of the accoucheur in this extreme degree of diſtortion of the aperture of the pelvis, is wholly directed to the preſervation of the life of the mother, as every endeavour within the limits of our art, when tending to preſerve the fœtus in theſe ſituations, will be abortive; and how far the morality of the practice may be juſtifiable, when performed with the view of preſerving the life of the mother, and ſacrificing that of the fœtus, is not a matter eaſily determined; the well-known aptitude inherent in a woman to conceive under theſe circumſtances, and the number of fœtuſes eventually deſtroyed with the intent of preſerving the life of the parent, are incidents of the utmoſt importance to the community, and claim a proportionate ſhare of conſideration from every accoucheur concerned on theſe unfortunate occaſions. In a ſituation ſo depending, is the accoucheur excuſable who tacitly complies with the requeſts of the mother, and voluntarily ſacrifices a number of immature fœtuſes with a view to her own preſervation? As the Cæſarean operation in this ſituation may be made a matter of choice, may it not be allowable for the mother to exerciſe her judgment on the occaſion? In certain caſes, where the diſtortion of the pelvis is not very conſiderable, it is a practice adopted by ſome practitioners, as ſoon as the hand is admiſſible into the uterus, to return the preſenting head of the fœtus, and deliver by the feet: how far this mode of practice may be juſtifiable, I cannot pretend to ſay, as I am at a loſs to conceive how any material advantage can be obtained by reverſing the head, except it be where the uterine efforts are too feeble to accompliſh the exit of the fœtus in a natural preſentation, and where the advantage gained ariſes from the ſuperior degree of power excited by the accoucheur over that of the uterus. In caſes of this nature, and where the diameter

meter of the ſuperior aperture meaſures no more than three inches from pubis to ſacrum, the head of the fœtus will generally, by the repeated action of the uterus upon its body, be forced a certain diſtance into the brim of the pelvis. When this is accompliſhed, and the head is aſcertained to have advanced one-third within the ſuperior aperture, it will be adviſable to attempt delivery with the lever; for I have, in a few inſtances, ſucceeded with perfect ſafety to both mother and child with this inſtrument, even in caſes where the crotchet has been uſed in the preceding births: hence will appear the advantage of waiting till the head of the fœtus is become ſo far advanced in the pelvis, that the lever is admiſſible, rather than involving the life of the child in unneceſſary danger, by the implicit act of turning and delivering by the feet; a practice, even in the moſt promiſing ſituations, always attended with danger to the fœtus, and which has not hitherto been explicitly made clear by authors.

Another ſituation wherein premature delivery has been adopted, is in caſes of habitual miſcarriages at a certain period of geſtation, beyond which the fœtus is ſuppoſed to die. The practice of inducing premature labour while there remains a proſpect of the fœtus being alive, will involve the accoucheur in much ambiguity, particularly as the exact date of impregnation cannot clearly be aſcertained, nor can we altogether obtain a certitude of the life of the fœtus in utero, or whether the woman might not go to her full reckoning, till the art of midwifery has arrived at that acme of perfection, competent for the acquiring of the knowledge of theſe data. I think no one excuſable who attempts this ſpecies of delivery, as inſtances are not wanting, where, after a number of periodical premature births, the woman has, at length, gone to the full period of nine months, and become mater familias.

Authors appear divided in opinion reſpecting the danger incurred upon the mother, by exciting premature labour. The parity of hazard which reſults from this ſpecies of birth, on the ſcore of the woman, at the period of ſeven or eight months, will vary according to circumſtances, when contraſted with the event which will follow the termination of a labour at full time, and where the pelvis is diſtorted to the degree which I have fixed upon as indicable for this operation.

Were we to form an opinion on the event of a miſcarriage at the abovementioned period, we might be led to conclude, that little danger to the mother would follow this mode of delivery; but as the uterus is an organ in no reſpect governed by the will, and the efforts of that viſcus are moſt regular when left uninterrupted, therefore no compariſon will hold

good

good betwixt a premature delivery and one artificially produced, nor betwixt one at the full period of pregnancy where the pelvis is well formed.

An objection of much weight against this operation will naturally impress the mind of every humane accoucheur, namely, the difficulty of secrecy by which premature labour is effected; for I am firmly persuaded, if ever the method should be divulged amongst a certain class of individuals, it will soon become too generally known, and the abortive attempts will be innumerable; this will doubtless defeat the intention of the operation, and lead to a crime of a most cruel and inhuman nature.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

BEING engaged in a large share of Midwifery, and having delivered near two thousand women, the case I am to relate never occurred to me till very lately: As I do not recollect to have seen or heard of such an one, the insertion of it in your valuable publication may not prove unacceptable. It may be a means of saving the young accoucheur a good deal of embarrassment, to be informed, that such an occurrence will seldom cause any impediment to delivery, and that in no very great length of time the issue of it will be favourable. At a village near this place, I was called to deliver a woman of her seventh child, and who, I found, had been in labour some hours. Upon taking the necessary steps of information, I discovered the right labium different from what it should be, and enough to excite my attention; but not considering this had any thing to do with the present business, I went on with the examination per vaginam. The os uteri was sufficiently dilated to admit a part of the child's head into the pelvis. As the labour advanced, I had the opportunity of informing myself of the situation of the tumefied labium, which to my astonishment continued to increase. At length it acquired the size of a child's head just born, and became very tense, hard, and painful. About this period a considerable hæmorrhage took place, amounting in quantity, as far as I could judge, to a quart or more. I now began to be alarmed; and anxious to satisfy myself of this uncommon appearance of things, I made a more minute investigation, and saw the true state of the tumour; found it had burst, assumed a dark livid hue, and that the bulk was not so much diminished as we ought to have

have expected from the quantity of blood thrown out, and which evidently came from that quarter. The labour not advancing in proportion to the smartness of the pains, I suspected the pressure of the distended labium might prove the obstacle.— Upon which I was led, as well as from other circumstances, to support the tumour, and confine it in the best manner I was able to one side, so as to give as much room as the nature of it would admit, for the passage of the child's head. The labour was accomplished, and with much greater ease than I had reason to suppose. It may be right to notice a circumstance respecting both mother and child, that the one had a well-formed pelvis, and that the head of the other was of a moderate size; the woman likewise was robust and in full health. The length of time I was taken up with this patient, did not exceed much more than an hour, so that the accumulation of blood in the cellular membrane could not have been long collecting. As soon as the woman was settled in bed, she expressed more uneasiness from the weight of the tumour than from any thing else. The common applications that such accidents indicate, were applied, with a full direction to support the part in as convenient a way as could be thought of. The discharge continued for a few days in large quantities, and very offensive. On the twelfth day the labium had nearly got to its natural size; the discharge became more purulent, and at length the cure was completed without any difficulty whatever. The constitution never suffered in the smallest degree from this local complaint. Why a rupture of a vessel should happen here, and why it does not oftener occur, is not my design to inquire. I am more inclined to suggest a remedy, which I confess I should be disposed to adopt, were a similar case ever to fall to my lot again; I mean the scarifying the parts with freedom immediately on the coming on of the accident, with the view of preventing the great distention of the teguments and its consequences; but how far this would be conducive to the end designed, experience alone can decide. I am,

Gentlemen,

Your humble servant,

EDWARD HUMPAGE, Surg. &c.

Stroud, Glocestershire,
Nov. 26, 1800.

To the Editors of the Medical and Physical Journal.

Gentlemen,

At this time, when medical men are so much at variance concerning the treatment of burns and scalds, it behoves every one

one to contribute to the general ſtock of facts, upon which, rather than upon any pre-conceived theory, this new doctrine muſt be eſtabliſhed.—If you deem the following caſe ſufficiently intereſting to merit a place in your valuable Journal, by ſo doing you will much oblige,

GENTLEMEN,

Your humble ſervant,

CHAS. NEWBY WAWN.

Newcaſtle upon Tyne,
Dec. 3, 1800.

G. T. aged 14, ſtanding near a fire, with a caniſter which contained a conſiderable quantity of gunpowder in his hand, wantonly threw a few grains into the fire; the ſparks from which communicating with the uncovered caniſter, produced an exploſion ſo violent as to burſt open the windows and door of the room in which he was. His right hand and the whole of his face were ſeverely burnt, the cuticle wholly abraded, and veſications formed; the eye-laſhes, eye-brows and hair on the forehead were entirely conſumed; he complained of violent pain, and ſhivered inceſſantly; pulſe 120.

On firſt ſeeing him, which was about twenty minutes after the accident, gts. xxx of laudanum were immediately adminiſtered, the ſpt. terebinth. was liberally applied to the whole of the inflamed ſurface, and above that, plaſters ſpread with Mr. Kentiſh's digeſtive ointment (Ung. reſin. flav. reduced to a thin conſiſtence with ſpt. terebinth.). He was ordered to be put to bed. This occurred in the forenoon; I ſaw him again in the evening, when he declared himſelf quite eaſy and comfortable; had ſlept two or three hours, and during the afternoon had felt only at times a paroxyſm of pain, which was much leſs ſevere than at firſt, pulſe 112. A draught with gt. xx of laudanum was ordered to be taken at bed-time. Second day, has paſſed a tolerably good night; the inflammation on the face appears leſs vivid. In removing the dreſſing from the inner part of the hand, the veſications broke, and a portion of the detached cuticle came off with it; from which, however, no unpleaſant ſymptom occurred; pulſe 105. Spt. terebinth. more ſparingly applied, other remedies continued. Third day, is materially better; has had a diſcharge from the angles of the eyes, which he has now opened for the firſt time; they appear ſlightly inflamed; purulent ſecretion beginning to take place on the hand, and partially on the face; pulſe 95. Draught omitted.

From this time ſtimulant applications were gradually withdrawn, and the parts were healed with cerat. lap. calam. which was completely effected in twelve days from the accident. The face is not in the ſmalleſt degree disfigured, nor is the ſlighteſt veſtige of the burn to be ſeen on the hand, of which he retains the perfect uſe.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

ENCLOSED you will find some remarks upon the nature and treatment of Diabetes; they are the result of an impartial and attentive inquiry; and if you think proper, I wish them to be inserted in the Medical and Physical Journal.

I am,

GENTLEMEN,

Norwich, Dec. 8, 1800. Your obedient servant,

RICHD. LUBBOCK.

In the history and treatment of the morbid actions of the living body, experience tells us there is much difficulty in tracing any given effect to its appropriate cause; and the source of this difficulty will, by the reflecting mind, be readily and truly referred to the intricate structure of the human machine, acted upon by a variety of causes, whose influence from their nature can be but remotely inferred, and which must of course frequently either escape or delude the eye of the nicest observer.

I have been led to this train of thinking, from the too general disappointment which medical characters suffer, in reducing to practice modes of treatment, in the opinions of their authors and inventors, said to be effectual and certain in the removal of certain diseases.—Many years have not elapsed, since the medical world was cheered with the prospect of relieving cancerous affections by the aid of cicuta; and for some time we have been led to suppose mercury an antidote to hydrocephalus.—To enumerate similar or more recent instances will be unnecessary; it will be sufficient to remark, that within the last two or three years I have experienced a similar disappointment, in my attempts to relieve several diabetic patients who have fallen to my care; and from what I have seen of this disease, I am led to think, that its mode of cure still remains to be discovered.

In the full and ingenious publication of Dr. Rollo, a mode of treatment, said to be efficient in diabetes, has been given to the medical world; but I think, in the subsequent observations, it will appear, that this mode has proved unsuccessful in many cases, although its application has been conducted with candour, and carried on with perseverance.

I shall proceed therefore to detail the history of some cases of this disease, which were taken into the Norfolk and Norwich hospitals, the event of which, it would seem, has been unfavourable

vourable to Dr. Rollo's theory and treatment of it. And from the obſervation which I have been able to make upon this diſeaſe, I think I am warranted in ſaying, that a leaning towards what is marvellous has pervaded, and in ſome meaſure diſtorted, the hiſtory of its ſymptoms and phenomena from the earlieſt times.

Thomas Smith, æt. 39, by trade a ſhoemaker, was admitted into the hoſpital, Dec. 21, 1799; he complained of an emaciation, which, he ſaid, had commenced about two years, and which had increaſed more particularly during the laſt twelve months. From his ſhrinking, the outlines of the face had become ſo ſtrong and prominent, as to convey to the moſt common obſerver, a ſtate of great diſeaſe. His ſkin was harſh and dry, and apparently altogether impervious to the perſpirable fluids; he complained of great thirſt, and of a ſenſation of heat about the ſtomach; and upon inquiry, ſaid he made a large quantity of urine. His appetite was good, and the abdomen was ſomewhat enlarged, but there was no particular irregularity of bowels. He had no affection of the prepuce, but complained of weakneſs of the loins; his ſight was alſo weak, and the verge of the eye-lids was red and ſlightly inflamed, and the gums were ſpongy and ulcerated. He had no particular pains. From the emaciation, the direction of the muſcles and the courſe of the veins over the whole ſurface of the body were ſo diſtinct as to denote the total abſence of fatty matter. But notwithſtanding this appearance of diſeaſe, he poſſeſſed conſiderable muſcular power (a circumſtance peculiar almoſt to diabetic macies), and had been able to work at his buſineſs. His mode of living had not been marked by great intemperance, and he had only lately, from thirſt, indulged in a freer uſe of malt liquor and other liquids. Having ſeen him a week previous to his admiſſion into the hoſpital, I deſired him, during that interval, at different times, to meaſure the quantity of urine paſſed in twenty-four hours, and he found it vary from twenty-nine to thirty-one pints. After his admiſſion, his urine was regularly collected, and meaſured in the preſence of the houſe apothecary, every twenty-four hours, beginning from Sunday noon, Dec. 22. The following table will exhibit the quantity of urine paſſed daily, for the firſt few days; but a daily report being tedious and unneceſſary, afterwards the greateſt and ſmalleſt quantities paſſed during longer intervals are only noted. The figures above the line denote the dates; below the line, the quantity of urine paſſed. There will alſo be found ſubjoined, tables of the urine paſſed by two other diabetic patients: of Richard Foſter, æt. 37, by trade a carpenter, who was admitted into the hoſpital, January 26, 1800, and who previouſly paſſed about 12 pints of urine daily;—of James Brown, æt. 43, by trade a weaver, admitted

April 6, 1799, who paffed from 10 to 14 pints daily, previous to admiffion.

SMITH.

Urine paffed in 24 hours previous to admiffion.	Dec. 22 to 23	24	25	26	27	28 to 31	Jan. 1 to 31	Feb. 1 to 28	Mar. 1 to 24.
Pints 29 to 31	18	$13\frac{1}{2}$	$9\frac{1}{2}$	8	6	5	$4\frac{3}{4}$ to 4	$4\frac{1}{2}$ to 4	$4\frac{1}{2}$ to 4

FOSTER.

Urine paffed in 24 hours previous to admiffion.	Jan. 26 to 27	28	29	30	31	Feb. 1 to 28	Mar. 1 to 23
Pints 12	$9\frac{1}{2}$	$8\frac{1}{2}$	8	8	7	6 to $4\frac{1}{2}$	5 to $3\frac{1}{2}$

BROWNE.

Urine paffed in 24 hours previous to admiffion.	April 6 to 7	8	9	10	11	12 to 30	May 1 to 26
Pints 10 to 14	6	$5\frac{1}{4}$	$4\frac{1}{4}$	5	$4\frac{1}{4}$	$3\frac{3}{4}$ to $1\frac{1}{2}$	3 to 2

Of thefe patients, two were of what is termed the fanguine temperament, having fair complexions and light hair; the third was fomewhat florid, but had dark hair. I have mentioned the particulars of Smith's fymptoms, his cafe being the more ftrongly marked; and as the fymptoms of the other two cafes differed from his only in degree, excepting that Browne's prepuce was affected with phymofis, and Fofter's gums were not affected, I was of opinion any detail would be ufelefs. It fhould be obferved, that the urine of all thefe patients was whey-coloured, fweet to the tafte, and a violet fmell.

The fame plan of diet was purfued by all thefe patients; they were all allowed four pints of liquid daily, confifting of two pints of milk, and of two pints of meat decoction, for the moft part beef, without any oatmeal or vegetable matter, and they had meat twice daily, viz. for dinner, and a fmaller portion for fupper. Bread and beer and every article of vegetable preparation were prohibited, and I have the greateft reafon to fuppofe thefe regulations were ftrictly adhered to. It muft be obferved, the milk and meat decoction were meafured with a common tin beer meafure, and the meafure by which the urine difcharged was eftimated was apothecary's meafure; and upon a comparifon I found the pint beer meafure ufed, contained an ounce and a half more than the apothecary's pint; fo that the four pints of liquid daily taken, would exceed in quantity four apothecary's pints of urine difcharged by fix ounces, or be equal to four pints

fix

ſix ounces; and it muſt alſo be recollected, in addition to the four pints taken, that theſe patients daily took from ſix to eight ounces of water, as a vehicle to the medicines which were thought neceſſary; ſo that the ſum of the liquid daily taken may be ſaid to be nearly equal to five pints, apothecary's meaſure.

The medicines taken were kali præparatum, natron præpar. aqua ammoniæ pura, kali ſulphurat. ammonia hepatizat. ferrum ammoniacale, &c. and they were given in full doſes, and for the moſt part under the inſpection of the apothecary.

This detail being premiſed, I ſhall proceed to offer a few remarks, naturally ariſing during the treatment of theſe caſes, which, if well founded, may, by an application to Diabetes in general, afford us more preciſe and accurate notions of it.

The circumſtance more immediately and ſtrongly aſſailing the attention, after the admiſſion of theſe patients into the hoſpital, was the immediate and ſudden reduction of the urine, which, as may be ſeen from the annexed tables, took place in a very few days, and in a degree ſo as not to exceed the quantity of liquid taken. This change taking place ſo rapidly, and before the quality of the diet ordered, or the effects of the medicines preſcribed, could have influenced the ſyſtem, awakened in my mind no ſmall doubt of the truth of a propoſition concerning this diſeaſe, which has been univerſally admitted as true, from the time of Aretæus to the preſent day; viz. that in this diſeaſe, the diſcharge of urine, in any given time, exceeds the liquid taken in the ſame time. The facts ariſing from the caſes above ſtated, tend in my mind to weaken the truth of this propoſition, by ſhewing, that the quantity of urine paſſed bears a more direct ratio to the quantity of liquid taken, and by no means exceeds it. That this is the caſe, I infer from the fore-mentioned tables; and from our ignorance of any proceſs or law of the animal œconomy, by which the liquid taken can, as ſuch, be augmented in quantity; and laſtly, from the various modes of treatment and theories of this diſeaſe, which have been foſtered by the imagination, with the ſole view of preventing and explaining this multiplying proceſs.

On this ſubject phyſiology affords us no aſſiſtance, and accurate obſervation tells us, that the baſis of all the liquids either remaining in, or diſcharged from, the animal machine, muſt have been received from without; and an attention to the foregoing tables will ſhew, that after the firſt few days, the urine daily diſcharged, continued, for weeks and months, to be leſs than the liquids taken.

From theſe facts and reflections, I am induced to deny that part of the hiſtory of this diſeaſe, that ſays the quantity of urine paſſed in a given time, ſurpaſſes the liquid taken.

 And

And in carrying our inquiries farther, and to the origin of this ſuppoſition, it is rendered probable that it commenced in the obſervations, either made by Aretæus himſelf, or received by him from others, he having expreſsly and in terms ſaid, "that the quantity of liquid taken is not equal to the quantity of urine, for that there is more urine paſſed." I am fully aware, that Aretæus is ever quoted as a model of preciſion and accuracy in the detail of his hiſtory of diſeaſes; and to doubt what antiquity has ſanctioned, and habit has confirmed, may be thought a bold, perhaps an invidious undertaking; yet, I hope I ſhall not be charged with want of candour, when I ſay, from a review of the hiſtory of Diabetes, as delivered by this author, that it is more than probable his wonted judgment has not been exerciſed in the detail of its ſymptoms, either from the little opportunity which he had of ſeeing it, or from ſeeing it with a mind benumbed by the impulſe of aſtoniſhment. That ſuch is the caſe, his firſt remark will evince, in which he ſays, "that a wonderful phenomenon is the affection of Diabetes, and very rarely happening to man." And afterwards in explaining more particularly the cauſes of this diſeaſe, and conſidering it "as a deliqueſcence or converſion of the ſolids into a liquid form," it would ſeem, that Aretæus found, to account for, and get rid of the ſolids ſo converted into liquid, a ſecond ſuppoſition was eſſentially neceſſary, giving origin to the opinion in queſtion, "that the liquid diſcharged by urine exceeded the liquor ingeſta."—This ſeems to me the origin of an error in the hiſtory of this diſeaſe; and to this ſource the fanciful details of caſes, the ſtrange perverted relations in the collections of Schenkius, as well as of others, may be truly traced.

If, however, it be proved, that the urine diſcharged does not exceed in quantity the liquid taken, an important concluſion readily preſents itſelf to the mind, which is, that the emaciation taking place in this diſeaſe, does not ariſe from any loſs ſuſtained by an undue or increaſed quantity of urine beyond the liquid taken; and therefore, ſome other cauſe is neceſſary to the explaining this phenomenon. On the ſame ground it may be obſerved, that the modes of treatment, inſtituted ſolely with a reference to the increaſed quantity of urine, are erroneous; and as ſuch, the rubbing the ſurface with oil or unctuous ſubſtances, to preclude abſorption from the atmoſphere, is unneceſſary and inefficient, as reſulting from a principle falſe and imaginary. Neither, to explain this ſurplus of urine, is the ingenious conjecture of Dr. Rutherford better founded, conſtituting the lungs an elaboratory of water, and giving them the function of forming water, by the union of the hydrogen of the blood with the oxygen of the atmoſphere. And having rendered it improbable

that

that the emaciation of Diabetes is connected with the quantity of urine diſcharged, I ſhall proceed to conſider the quality of Diabetic urine as marked by a ſaccharine or honied taſte. This quality, obſerved firſt by Willis, has attracted the attention of later authors, and more particularly of Dobſon and Rollo, who finding that a greater quantity of extractive matter is produced by evaporation, from a given quantity of diabetic than from the ſame quantity of healthy urine, have conſidered this ſaccharine product as a probable cauſe of diabetic emaciation. But it may be doubted, and perhaps with juſtice, whether the increaſed portion of ſaccharine extract in diabetic urine, be a cauſe adequate to the explanation of the ſtate of ſhrinking, and whether it be not rather an effect or coincidence than a cauſe.— In determining this queſtion, it will be neceſſary,

1ſt. To compare the extractive matter produced, by evaporating equal quantities of diabetic and of healthy urine.

2nd. To compare the quantity of extractive matter obtained from a given quantity of diabetic urine, with the extractive matter contained in the ſame quantity of liquid taken. And,

Laſtly, To determine whether the concurrence of any other cauſes may not contribute to the increaſe of extractive matter in diabetic urine, without ſuppoſing that the deſtruction of the ſolids contributes to ſuch increaſe.

With a view to the determination of this queſtion, a pint of Smith's urine was evaporated, at four different times, and the extractive matter was carefully collected; viz. a pint was evaporated immediately upon his admiſſion into the hoſpital, at the period when he was paſſing about thirty pints of urine daily, and, as near as may be, an ounce of ſaccharine extract was produced. In about a week, or as ſoon as the urine was reduced to the limited quantity of liquid ordered, another pint was evaporated, yielding of extract fourteen drachms. In a month or five weeks after the adoption of his curative plan, a third pint gave only ſix drachms of extract; and about a week before his diſcharge, another pint gave about an ounce of extract.

To explain the fourteen drachms of extract found in evaporating the ſecond pint, it may be obſerved, that this experiment took place as ſoon as the urine paſſed was reduced ſo as not to exceed the quantity of liquid daily ordered, and therefore it was probable that all the extractive matter contained in the greater quantity of liquid taken, previous to his admiſſion, when he indulged his thirſt (not to notice the ſolid), was not yet eliminated from the ſyſtem; ſo that the liquids taken, being abridged to a given and much ſmaller quantity, there was found, for a ſhort interval, in every pint of urine, a greater and unuſual

uſual proportion of extractive matter. This explanation being admitted, it will appear, that the extract of Smith's urine, accurately ſpeaking, varied only from an ounce to ſix drachms.

To find the difference between this quantity of extract and that found in healthy urine, five pints of urine taken from healthy perſons were cautiouſly and ſeparately evaporated, and it was found the extract produced, varied from half an ounce to rather more than ſix drachms for each pint. Hence then it would appear, that the difference of the extractive products of healthy and diabetic urine is not ſo great as to afford a probable cauſe of the emaciation in diabetes;—and this will appear more fully hereafter.

But, in ſpeaking upon this ſubject, permit me to ſay, that a ſingular concurrence of events, but remotely connected, has caught my attention in this diſeaſe; I refer to the conſtant coincidence of the parched and unperſpiring ſkin with the ſaccharine quality of the urine: this concurrence has induced me, at times, to believe that in this diſeaſe the want of perſpiration, or of the excretion from the ſurface, was connected with the ſaccharine quality of the urine; and this belief has received farther ſupport from an examination of the nature of the perſpirable fluids, and of the component parts of ſaccharine matter. It has been proved that ſugar is, for the moſt part, compoſed of carbon, oxygen, and hydrogen united in a certain ratio; and it appears, by the experiments of Cruikſhank and Abernethy, that, beſides the occaſional aqueous fluid diſcharged daily from the ſurface of the body, about three gallons of carbonic acid are alſo loſt to the ſyſtem by the perſpirable matter. Now ſuppoſing, as happens in diabetes, this perſpirable excretion, or carbonic acid, is ſuppreſſed and retained in the ſyſtem, it is probable that the carbon and oxygen of the acid ſo retained by entering into a due combination with ſome portion of the hydrogen of the animal body, may tend to the production of the ſaccharine matter of the urine; and as the carbonic acid is the general product of the vegetable world, it would follow, that its retention, in the animal body, may produce the phenomena of the defective animalization, characterizing diabetes, as exhibited in the formation of ſugar.

Thus, in the preceding remarks, have I attempted, and I truſt with impartiality, to invalidate certain data concerning the hiſtory and theory of diabetes; I have attempted to diſprove the generally received notion, that in diabetes the urine is diſcharged in a greater ratio than the liquid taken; and I alſo truſt I have rendered it improbable that the quality or quantity of the extractive matter diſcharged by urine, is a principal cauſe of the emaciation; having ſhewn that ſuch extract may more juſtly

justly be referred to that contained in the liquid and solids taken into the body; and having made it probable that its saccharine quality may depend upon, or be connected with, the suppressed perspirable matter. Hence then it is evident, that any mode of cure, or plan of treatment, instituted solely with a reference to the increased quantity, or altered quality of the urine, must be defective and unsuccessful; and therefore, proceeding upon these erroneous principles, it will cease to be a matter of surprise, that, in the above cases, I should have adopted the suggestions of Dr. Rollo, without the advantage I had been taught to expect. In the three cases given above, a diet consisting solely of animal food, with the free and full use of the different alkaline medicines, including a liberal use of hepatized ammonia, effected little or no change in the general health. —And in Smith's case, although these regulations of diet and medicine were rigorously attended to, I cannot say that any great or observable change was induced in the quality of the urine, which seemed, in a great measure, to retain its saccharine taste and violet smell. And in this case, supposing that the milk taken might keep up these appearances, instead of the two pints of milk two additional pints of strong meat decoction were substituted, so that nothing but highly animalized food was taken; and under this plan he remained some weeks, but without any real advantage. At length, after a due perseverance, and becoming tired of the little variation of his food, his diet was ordered, on a sudden, and without any gradation, to be changed to the common hospital diet, consisting of bread, vegetables, meat, malt liquor, and milk pottage, adhering however to the taking, in liquids, only four pints daily. By this alteration of the nature of the liquids and solids taken, no marked change in the quantity or quality of the urine was induced, and he remained in the hospital some time without any change in the urinary discharge.

But it may be said, that this patient had been in the use of animal food so long, that his system was rendered proof against the influence of vegetable or mixed diet, as shewn by increasing the quantity or the saccharine quality of the urine;—in answer to which, it must be permitted me to remark, that Foster, another patient, had been, at the time in which Smith's diet was changed, only about a fortnight in the hospital, and had only for that time pursued the animal diet and alkaline medicines, and the urine discharged was in the ratio of the four or five pints of the liquids taken, as the table will shew; and to determine what change would be induced in the urine discharged after so limited an use of animal food and alkalies, he was also ordered, on a sudden, to take the usual diet of the house,

houſe, conſiſting of vegetables, bread, malt-liquor, meat, &c. but, on a cautious inquiry, it did not appear that the urine was increaſed in quantity; and in quality, ſo far from exhibiting a more wheyiſh or ſaccharine appearance, it was evidently, for ſome days, of a higher or more amber colour.

After this ſtatement, the power of a vegetable or mixed diet, in reproducing diabetes, by increaſing the quantity, or altering the quality of the urine, may with reaſon be doubted.

In the treatment of diabetes, patients are led, by the attention given by medical men to the quantity and quality of the urine, to ſuppoſe the ſole cauſe of their diſeaſe to be connected with the ſtate of the urine; and, therefore, whenever either the quantity of urine is obſerved to be reduced to the natural quantity, or the quality of the urine is deemed more favourable, they fall into the greateſt deluſion reſpecting their general health, by aſſerting in terms that they are becoming well, or free from diſeaſe, although, at the ſame time, the degree of emaciation is evidently growing worſe and advancing. This deluſion was ſtrongly exemplified in the three caſes before mentioned, their language being, from the decreaſe of their urine, that they had loſt their diſeaſe, although at the ſame time they were gradually loſing fleſh; and it is not to be doubted, but that, at no diſtant period, they will fall victims to the emaciation. And to the ſame end, it may be obſerved, in the various caſes reported in the accurate publication of Dr. Rollo, that very few, if any, excepting Capt. Meredith, ſeem to have regained their fleſh, although, from the urine being in quantity abridged by an abridgment of the liquids taken, they were deemed to be cured. This continuance of the emaciation may be remarked in the caſe of Clarke, ſo accurately reported by Dr. Gerard; for this patient's urine being reduced, he was diſcharged as cured by his phyſician, even at a time when his emaciation was ſtationary, or even increaſing; for being weighed on the day of admiſſion into the Liverpool Hoſpital, and upon the day of his diſcharge, it was found his weight had decreaſed one pound. And the continuance of the macies in the caſes ſo well deſcribed by Dr. Cleghorn, and in the other caſes reported in Dr. Rollo's book, proves the diſeaſed ſtate to have been unaltered, and to have reſiſted the curative means made uſe of. And I have no heſitation in ſaying, that it is probable, ſince the appearance of Dr. Rollo's book, containing thoſe caſes, many of them are ſuffering from greater emaciation, or may have paid the debt of Nature: And it would be an uſeful document to lay before the public, could Dr. Rollo procure from his correſpondents the particulars of every caſe that he has printed, ſhewing their ſubſequent progreſs; by which it might

might be known in what degree we may expect a fatal or salutary termination of this disease.

From all that has been said, it would seem, that the leading circumstance in diabetes is the macies, which, from the experience which I have had of this disease, frequently comes on slowly and tacitly, the subjects of it not perceiving or not feeling much deviation from health: In this way, I have known persons for three or four years affected with diabetes, but possessing a degree of strength equal to the various exertions of pleasure or of business; and under this degree of diabetes, I have, at this time, two patients, who, excepting a slight and very gradual wasting of the body, some increase of thirst, with occasional uneasy sensations about the stomach, to which may be added, feebleness of sight at times, and an habitual tendency to phymosis, possess that degree of apparent health and strength that would mislead a common observer. And to treat patients with so slight a deviation from health, it must be allowed, is an arduous and difficult enterprise; plans of diet may be prescribed, and medicines may be ordered, and the nature of their complaint may be duly pointed out to them; yet, irregularity in their application will necessarily occur, and a frequent violation of all the necessary salutary proceedings will take place; and, perhaps, from this cause it has happened, that I have not found even this incipient state of macies in diabetes removed; and when it is more advanced, I have never seen it yield to any curative measures, but it has continued to increase more and more, till, after a lingering and protracted wasting of the solids, the unhappy sufferer sometimes displaying a countenance the most hollow and hagard, with the different members, as it were, barely enveloped with the skin, at other times presenting an aspect bloated with anasarcous swellings, sinks gradually into the arms of death. And, as has been before observed, in this disease considerable muscular power remains with great emaciation; and this circumstance, I believe, has led very judicious practitioners to conclude, that diabetic patients have been materially relieved by certain curative means. But it is the emaciation that claims the first attention of the physician, for, without the removal of this symptom, neither the reduction of the urine, the alteration of its quality, nor the appearance of considerable remaining muscular power, will prove that the disease has yielded to any curative process.

These observations may have been lengthened beyond the limits intended; should they, however, in any degree tend to convey to the mind, notions more accurate of the nature and treatment of diabetes, my principal object, the promotion of medical

medical ſcience, will be attained. And I ſhall now only ſubjoin a few general concluſions; and,

1ſt. From what has been ſaid, it would ſeem, that in the noſological arrangement of diabetes, authors have materially erred, as it ſhould neither be claſſed with Cullen among the ſpaſmi, nor with Sauvage and others among the fluxus and profluvia; according to Cullen's arrangement, it ſhould fall under the claſs cachexia, and in the order marcores; and were I aſked to give a definition of it, I ſhould give the following, keeping in mind the conjecture of the connection of the retention of perſpiration with the ſaccharine quality of the urine as cauſe and effect: I ſhould define it,

Macies (anidrotica) cum cute non perſpirante, cum urina mellita, et plerumque multa ex multo potu, et haud raro cum palpebris, præputio et gingivis inflammatis.

And as from experience it would ſeem that this ſpecies of macies has hitherto admitted of no ſubſtantial relief from medical treatment, I am diſpoſed to conſider it with Selle and Neitzkius, as a diſeaſe of the lymphatic ſyſtem. It muſt be underſtood, that I am here ſpeaking of what may be termed idiopathic diabetes, and not of that ſpecies which Sydenham and others have mentioned as ſupervening as a ſymptom or ſequela of acute diſeaſe, and which is of ſhort duration.

2d. It is proved, that in diabetes the urine diſcharged is in a direct ratio of the liquids taken; ſo that in the caſes reported by Dr. Rollo, it would ſeem, as the macies remained unaltered, that all that was effected by the curative plan was, by abridging the quantity of liquid taken to abridge the urine diſcharged.

3dly. And if the urine diſcharged follows the liquid taken, it is evident the various alkaline preparations cannot influence the quantity of the urine; and from what has been ſaid, it may be much doubted, whether the quality of the urine is materially affected by them.

Laſtly. The occurrence of phymoſis has been mentioned by my friend, Dr. Girdleſtone, as a conſtant or pathognomonic ſymptom of this diſeaſe; but I find this ſymptom is dependent upon the formation of the prepuce, for in Smith and Foſter, the worſt caſes which I have met with, this ſymptom was abſent. This ſymptom has, however, taken place in four other caſes which have fallen to my care, two of which poſſeſſed ſo much apparent health, that the obſtinacy of this ſymptom afforded a clue to the unravelling of the nature of the complaint.

And in treating of the extractive matter of diabetes, authors ſeem to have confined their attention to the condition of the urine,

urine, or of the liquid difcharged, confidering fuch matter as derivable folely from the fyftem, without carrying their thoughts to the examination of the extractive matter contained in the liquid received and taken into the body. Hence, if Smith paffed thirty pints of urine daily, each pint yielding an ounce of extract, it would feem as if they concluded, that thirty ounces of extractive matter were taken from the fyftem daily. But let it be remembered, that this man took daily thirty pints of liquid, confifting of porter and other malt liquor; of milk, water, &c.; that his appetite being good, he took freely of bread, meat, vegetables, &c.: Then fuch liquids and folids will readily afford a caufe adequate to the production of the thirty ounces of extractive matter contained in the thirty pints of urine paffed.

Hence it is probable, the quality of the urine is not the caufe of the fhrinking, for the origin of the extractive matter found in the urine may be referred to that taken in by the liquids and folids, without recurring to any lofs fuftained by the folid compages of the body and its confequent emaciation.

Farther, thefe arguments againft the faccharine matter as a caufe of the fhrinking in diabetes, will receive a confiderable acceffion of ftrength, from a comparifon of the nature and quantity of the liquids and folids taken daily by Smith after he had been in the hofpital fome time, with the quantity of extractive matter difcharged by urine daily. In this cafe, two pints of good milk, and two pints of ftrong meat decoction, were daily taken; and alfo for dinner, not lefs than from fix to eight ounces of boiled beef or mutton; and for fupper, not lefs than from three to four ounces of the fame meat were allowed. And after he had ufed this plan for fome time, it was found by experiment, he difcharged daily from four pints to four pints and a half of urine, each pint yielding only fix drachms of extract; fo that, in twenty-four hours, little more than three ounces of extractive matter were difcharged, a quantity which by no means equalled the extractive matter contained in the milk and the liquids taken daily; and therefore fome portion of the extractive matter of the liquids, and the whole of the folids, might have been added to the fyftem, and might, notwithftanding the difcharge by urine, feemingly have anfwered all the purpofes of nutrition, and might, of courfe, have acted as a barrier to the increafing emaciation.

And, moreover, that the lofs of fubftance in Diabetes is not connected with any quality of the urine is farther fubftantiated, when it is confidered, that allowing fome fmall increafed difcharge of urinary extractive matter in thefe cafes beyond what takes place in health, it is well known that fuch patients are

K 2 fayed

saved any loss from the fluids or solids of the body by the absence of the wonted excretion of the surface, perspiration sensible and insensible being as it were annihilated, from the torpor and total inaction of the perspiratory vessels. And hence it would appear, by the defect of perspiration, a quantity of fluid and its extract is saved, and retained in the system, and perhaps more than a counterbalance to any increased quantity of extractive matter discharged by urine: And by this train of thinking, I have been induced to conclude, that the quality of the urine cannot be considered as the cause of the shrinking in Diabetes.

Case of Blindness from Effusion of Blood in the Chambers of the Eye, from suppressed Catamenia, cured by Electricity; by Mr. RICARDS, *Surgeon, of Brentford.*

To Dr. BRADLEY.

SIR,

THE following case being such, I think, as deserves the attention of the public, I beg leave, through the medium of your Journal, to make it known.

Miss ——, aged 17 years, after a troublesome head-ache, which continued several days, became nearly blind of one eye, so much so that she could only distinguish light from darkness; and every object which she attempted to view, appeared of a reddish hue. Her friends, much alarmed at this sudden accession of so terrible a malady, as the eye had to them a most frightful appearance, immediately made application to the most eminent of the faculty, particularly to such as made the diseases of the eye the principal part of their practice. By accurate examination, it appeared that the chambers of the eye were filled by an opake, dark-coloured fluid, instead of the transparent aqueous humor, and the whole eye appeared suffused with blood. Considering the disease as local, such remedies were directed as are generally employed to diminish local plethora, and to promote the absorption of the effused fluid. Leeches were applied to the temple, cathartics, blisters, &c. and she continued to follow these directions, and to pursue this plan of treatment for several months; but as no amendment was perceptible, she became weary of a course from which so little benefit was derived; at length, she was advised by a friend

to

to try the effects of electricity, who requested her to wait upon some practical electrician, and desire him to pass a stream of the aura from a wooden point into the eye daily; which she did accordingly.

The nature of the case being very fully inquired into (for, being consulted, I never chuse to make use of any means without knowing a reason why), and the progress of the disease, and the methods hitherto used, being very satisfactorily explained by the young lady's mother, it appeared that the blindness was more considerable at some times than at others, previous to which there was generally a considerable head-ache; and that these occurrences for the most part took place about every third or fourth week; and that the disease had from the beginning been attended with obstructed catamenia, which circumstance had been very much overlooked by the oculists who had been previously consulted; but though no menstrual evacuation had taken place, the usual forerunning symptoms had generally occurred. If we stop a moment to theorize upon the disease, it would seem, that Nature had made an effort to perform her regular office; but from some cause of inactivity in the uterine system, the proper catamenial secretion had not taken place, in consequence of which the superabundant blood had fallen upon some other part; the vessels, in this instance, which secrete the aqueous humor had become enlarged and ruptured, and pure blood, or blood mixed with the aqueous humor, had been poured into the chambers of the eye. The peculiarity of this case is in the part on which Nature has directed her efforts; we frequently see her pouring out her periodical discharges by the lungs, by the vessels of the Schneiderian membrane, or by ulcers in different parts of the body. In such cases, it appears to me that we should not apply our remedies to the eye, the nose, the lungs, or the ulcer, but to the uterus; for if this organ can be excited to a proper secretion, the evacuations from the other parts will soon cease: The sequel of the present case will prove this to be true, in this instance at least.

I consider that the remedies employed to produce this effect on the uterus should be made use of about the time when the usual symptoms which precede menstruation begin to appear; therefore, in order to divert the patient, but without expecting any real benefit from such a mode of application in a case of this kind, she had, daily, streams of electric aura passed into the eye, and likewise small shocks through the pelvis, waiting till symptoms indicated an effort of Nature to perform her proper office.

When such symptoms began to appear, as pain in the head and

and loins, tension of the breasts, &c. the shocks were greatly increased both in number and strength, without informing her of any intention of making such an increase of power; in a day or two the catamenial evacuation took place in a proper manner, and the eye was visibly clearer; the electricity was omitted till the next periodical return of symptoms, when it was repeated; the catamenia again appeared, and the eye very speedily recovered its original transparency, nor did any return of obstruction take place.

I consider this case as one on which much may be said, relative to the good effects of electricity in uterine cases. I would not be understood from hence to recommend electricity in every case in which obstructed menstruation is a symptom; in some such cases I am well aware that its use would be injurious, rather than salutary. I would distinguish those cases in which amenorrhœa is a cause, from those in which it is an effect of the disease. Chlorosis, I consider as a disease of the latter kind; the symptoms, here, all indicate a paucity of blood, and Nature, in this case, acting as a judicious physician, prevents the loss of that fluid, whose deficiency is the very essence of the disease, by restraining this customary evacuation; it would therefore, I think, be highly improper to endeavour to force such an evacuation by any remedy which may be considered as emmenagogue, of which I certainly consider electricity to be the most active; nevertheless, I know that it is the practice of some gentlemen of the profession to advise venæsection, almost invariably, in this disease. But where there is suppressed menstruation from any accidental cause, where no symptoms of paucity of blood exist, or where Nature points to some other emunctory, or ruptures vessels in remote parts of the body, here the reproduction of the catamenia must be our chief concern; and to effect this, from repeated experience I can affirm, and I know it likewise to have been the result of the practice of others, electricity will effect more than perhaps any other remedy; and, if used in combination with other remedies, will greatly assist them in their curative intention. I have the honour to be,

Sir,

Your most obedient servant,

Brentford, Dec. 14, 1800. JASPER RICARDS.

To

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

SINCE the publication of thoſe obſervations, which I have ventured to call demonſtrative of the anti-venereal power of acids, I have received further teſtimonies. I encloſe a ſpecimen.

I have made ſome progreſs in a collection towards defraying the expence of a ſet of public experiments. Mr. Pearſon thinks with me, that a ſeparate eſtabliſhment is beſt ſuited to the deciſion of this queſtion. Among other regulations, it ſeems neceſſary for ſatisfying the public mind, that a perſon of unſuſpected probity ſhould be fixed upon to adminiſter the acids; that this perſon ſhould ſwear to the fidelity of his reports; and that the nurſe and patients ſhould ſwear, the one not to ſupply, or cauſe to be ſupplied, any article not directed; the other, not to take any ſuch articles.

It is not merely for the ſake of bringing our controverſy, important as it is, to an iſſue, that ſuch a plan is deſirable; the contradictory reports place before our eyes the exiſtence of a moſt criminal levity in obſerving a diſregard to truth in reporting. What human being, who conſiders that he himſelf, and thoſe he moſt values, are one day deſtined to become the ſubjects of medicinal proceſſes, but muſt tremble when he perceives that there muſt be, at the preſent time, in this enlightened country, a conſiderable number of reputable medical practitioners, incompetent to the deciſion of one of the eaſieſt and moſt palpable problems belonging to their art; who cannot, or will not uſe their ſenſes with a little coolneſs, patience, and impartiality? When ſuch a perſon is to be treated for an obſcure internal diſeaſe, in what hands ſhall he deem himſelf tolerably ſecure?

It appears from an hundred examples, that any man may publiſh almoſt any thing concerning any remedy, with ſome chance of a lucrative reputation, and at ſcarce any hazard of ruinous diſgrace. Not only, therefore, will a public ſcrutiny expoſe that party, which, in the diſpute concerning the acids, is chargeable with ſuch portentous incapacity; but it will ſerve as a wholeſome leſſon to thoſe who are ready to diſcharge their crudities upon the public.

For my own part, I feel aſſured that the inquiry will confirm the favourable atteſtations. And there are two arguments ariſing from a comprehenſive view of the documents on both ſides, which muſt ſtrike impartial judges. 1. The affirmative caſes were very open to inſpection; and at Plymouth, Woolwich,

wich, and in India, they were actually inspected by a number of medical men, in no respects biassed in favour of acids. 2. Among the assertors are persons equally versed in medical observation, and in experiments on inanimate matter; an exercise which habituates the eye to examine with accuracy, and the mind to reason strictly, and which ought to be made an indispensable requisite in medical education.

So much having been said in Europe on the subject of relapses, the East India practitioners have been induced to attend particularly to this objection. And Dr. Helenus Scott, in a letter too late for my last pamphlet, thus states the result:

"With regard to relapses, when recent cases have been cured by the nitric acid, I believe they occur less frequently than after mercury. Mr. Stuart, Dr. Keir, Mr. Macgregor, and myself, have cured by it, within these eighteen months, above one hundred patients with primary symptoms, one only of whom has relapsed. This man, a patient of Mr. Macgregor, came back to him with a small chancre on the prepuce, but it is doubtful if this did not arise from a fresh infection. No practitioner surely will assert that so great a number would have continued well, who in this country had been treated by mercury."

I am glad of an opportunity to mention that my pamphlet is much disfigured by errors of the press. In many places of the preliminary discourse, in spite of my corrections, the London printer has made me write strange English. There are few places, however, where the sense cannot be made out; and I hope by this time, that all the copies are provided with a list of errata. I only beg leave to mention here, that at p. 51, l. 21, *citric* acid should be read in place of *nitric*. I am,

GENTLEMEN,

Respectfully yours,

December 12, 1800. THOMAS BEDDOES.

To Dr. KEIR, from CHARLES FORBES, Esq.

My dear Sir,

Agreeably to your desire, I now send you the particulars of the cure effected by Dr. Scott on one of our servants, by the use of the nitric bath.

About a twelvemonth ago, the servant alluded to (a native boy of 16 or 17 years of age) was seized with violent pains all over his body, but particularly in his head and loins, which were most severe in the night-time, and entirely deprived him of rest, except what he procured with the assistance of tobacco or

or opium, he soon became reduced to a skeleton, and so ill that he had nearly lost the use of one of his legs and thigh, the muscles of which were so much contracted, that he could only put his toes to the ground, and his skin was covered with white scabs or scales. Hitherto he had persisted in denying that he was ever infected with the venereal disease, and attributed the pains in his bones, and contraction in his legs and thigh, to a stroke of the land wind, which was corroborated by the opinion of the other servants; he now confessed, however, that he had caught the venereal disease about a year before, which he described to have been a running and ulcers, and that he removed those symptoms by drinking milk, which had been recommended to him by one of his friends; but that he took no mercury, nor any other medicine. On describing the boy's case to Dr. Scott, he was so good as to visit him, and recommended the nitric bath, which was immediately commenced in the usual way, at first twice daily, and about half an hour at a time; but it was remarkable, that in the course of a week, the boy found himself so much relieved, that he repeated the bath half a dozen times in the day of his own accord, and at length would frequently sit two or three hours at a time up to the neck in it, until his skin became so sore that he could hardly bear to put on his clothes. His headachs had now left him, he rested well, and only complained of the pain in his loins, which, by a moderate use of the bath, was also removed in the course of about a month from the time he first began it, and he gradually recovered the use of his leg. He is now in perfect health, and has had no return of his complaints. He never took mercury, but his mouth was considerably affected by the use of the nitric bath.

I take this opportunity of mentioning two other instances of the efficacy of the nitric acid, which have come within my immediate observation. The first is that of John Hibernia, a Caffree, aged about forty, who was servant to the late Colonel Nugent, and whom Mr. Forbes put under Dr. Scott's care about two years ago; he was afflicted with venereal pains in his bones, for which he had been repeatedly salivated by mercury, but received only a temporary relief from it, for when the effects of the mercury ceased, his pains returned. Dr. Scott administered the nitric acid internally, and in the course of a few weeks completely cured him. I lately met him on the road, and on questioning him respecting his health, he informed me that he was perfectly well, and had been so for a twelvemonth past.

The second instance alluded to, is that of an officer of one of our ships, for whom I requested Mr. Boag's advice. Having accidentally

accidentally fcratched his finger, it turned into any ugly fore, which gave him exceffive pain; and although timely applications were made, it feemed to defy them all, and at length put on fo putrid an appearance, that Mr. Boag thought the bone was affected, and that the lofs of the finger muft enfue. He now recommended a trial of the nitric acid upon it; and by bathing the finger in it, three or four times a day, as ftrong as it could be borne, the fore was foon healed up, and the finger got perfectly well.

I remain your's,

Bombay, Jan. 1800. C. FORBES.

To the Editors of the Medical and Phyfical Journal.

GENTLEMEN,

IT will, I prefume, be gratifying to the numerous lift of phyficians and furgeons, who have patronized Dr. Jenner's difcovery, to hear that the inoculation of the vaccine difeafe is making rapid progrefs in the fleet. This bufinefs has been much facilitated by the kind attention with which the matter is fupplied to our furgeons, as they arrive from fea, by Meffrs. Dunning and Little, two eminent furgeons in this town. I embrace this opportunity, as the executive medical officer of the fleet, to offer them my beft and moft grateful thanks in the name of the public fervice. Mr. Stephen Hammick, of the Royal Hofpital, is alfo entitled to this public acknowledgment for fimilar kindnefs.

If the value of any improvement in the practice of medicine is to be eftimated by its contracting the fcope of human mifery, the JENNERIAN INOCULATION will be defervedly recorded as one of the greateft bleffings to the navy of Great Britain that was ever extended to it. Since the publication of my fecond volume of Med. Nautica, not lefs than eleven fhips of the line, befide frigates, have imported, from various fources, the variolous contagion.

In the flag fhip of the commander in chief, while refitting at Portfmouth, laft fummer, it extended to twelve cafes before it was checked. Dr. Felix, who then officiated as furgeon of the Ville de Paris, reports to me, that more men were found who were unconfcious of having had the Small-pox, than had been known in any other fhip of the line. One fhip of feventy-four guns, carried it to fea in the fpring, and buried five feamen during the cruife; another has carried it abroad, the fate of which

which I have not yet learned. The Impetueux, now in harbour, received the Small-pox from Dock, where they rage with conſiderable mortality. A lieutenant in this ſhip ſet the example of the new inoculation to the ſeamen; fixteen have taken it, and others, from ſuperſtitious motives, ſtill hold back.

When the candid enquirer, at ſome future period, ſhall canvaſs theſe obvious facts, it will be ſeen whether I was acting an inconſiderate part in recommending the practice in the navy, when it firſt came before the public, as malignantly ſtated by ſome cold-hearted critic of the preſent day.

In the ſpring of the year, when fevers of the low kind were rather general in our ſhips, the *affuſions of cold water*, as recommended by Dr. Currie, of Liverpool, were found of great ſervice by Meſſrs. Farquhar, of the Captain, and M'Grath, of the Ruſſel. Theſe gentlemen had their hands full for a length of time; but I am diſpoſed to mention the trials here, that the learned author may know that his advice is not thrown away upon us. Other particulars will appear, in due time, in our future Hiſtory of Health.

Since my laſt communication on Coxwell's Concrete Citric Acid, experiments have been directed by the Right Honourable Lords Commiſſioners of Admiralty, with their uſual benevolence, and the reports of ſeven ſhips uniformly confirm the firſt trials. This preparation of the lemon has now got to the meſs table of all our officers, and will, no doubt, become general. The ſubject of ſcurvy is thus finally diſcuſſed, unleſs Sir Francis Milman, Bart. ſhould produce ſome freſh arguments to ſupport his theory, in a new edition of his elegant work. The apothecary will alſo find a pure and permanent ſolution of this acid a valuable addition to the ſhop.

With reſpect to my own ſtudies, as well as a moſt cruel rheumatiſm will permit, I proceed as heretofore, like Horace,

Condo et compono quæ mox depromere poſſim.

I have not heard a word of the Nitrous Fumigation from the ſurgeons for theſe many months. The maſterly diſcipline enforced by the illuſtrious officer at the head of the fleet, has effectually ſubdued all wavering opinions on that ſubject. We ſhall, however, always be glad to receive hints from phyſicians at the weſt end of the town; and let them be aſſured, there is no prejudice here, but the utmoſt deſire to improve. I am,

GENTLEMEN,
With much deference,
Your obedient humble ſervant,
T. TROTTER.

Plymouth Dock,
Dec. 9, 1800.

Reply

Reply to Mr. Oliphant's Information on Mrs. Craib's Case of Cancer; by William Nisbet, M.D. Fellow of the Royal College of Surgeons, Edinburgh, &c.

To the Editors of the Medical and Physical Journal.

Gentlemen,

In the Nineteenth Number of your Journal was inserted a case of the cure of Mammary Cancer, as drawn up by Dr. Nisbet. The relation of that case has excited much attention, and much medical scepticism has been betrayed on the subject; to favour which, in your last number, a new statement has even appeared, under the title of *information* respecting the case of Mrs. Craib, by Mr. Oliphant, the attending surgeon. As the title of this statement is clearly meant to imply a reflection on the former relation, it is my duty to reply to it fully; and to show, that the facts *then* advanced, and the principles of treatment then detailed, were equally just, and deserving the notice of the public.

In making this reply, I shall first examine the description of the disease presented by Mr. Oliphant; 2dly, the principles assumed by him as effecting the favourable termination of it; and, 3dly, remark on the analogous cases cited by him in support of these principles.

With respect to the description of the disease, little difference occurs to be noticed in the two statements; indeed, the morbid appearances were so strongly marked and characteristic, as not to admit of any. At the same time it will be apparent to every unprejudiced reader, how much, even in this part, Mr. Oliphant has endeavoured to strain every fact to favour his particular opinion: Every real cancer he considers as incurable, and no disease he regards as cancer that is curable. Laying down then this axiom, the malignity of the morbid appearances in the present instance, which could not admit denial, he ascribes less to the specific nature of the malady than to preceding errors in its treatment. But in this attempt, like most people who deviate from the fair or candid line of conduct, he has placed himself in a most awkward dilemma, and by setting up his single ipse dixit, has opposed the opinions of some of the most respectable of the profession. He has found it necessary, therefore, to omit altogether the names of the gentlemen formerly concerned with the case, and whose sentiments he was made acquainted with by the patient, as well as Dr. Nisbet, sufficing

tiſficing himſelf with giving Mr. Cline the appellation of a competent ſurgeon, and Sir James Earle of an eminent and experienced one, but taking no farther notice of any other perſon. Whether theſe gentlemen will be more offended by their names being formerly noticed as having merely ſeen the caſe, or now being declared ignorant of its real nature, and as having paſſed an erroneous judgment upon it, it is not my buſineſs to determine: their names became introduced by me in conſequence of the patient's own relation, and *that* with propriety, in order that they might have an opportunity of proving the authenticity of my ſtatement. I conſider them alſo men of too much liberality and candour, and too much actuated by zeal for the improvement of ſcience, to ſuppoſe them capable of taking offence on ſuch grounds. The juſtice of their opinion I never could have a doubt of; for if ever there was a caſe of real idiopathic cancer, it was clearly exemplified in this inſtance. The diſeaſe had made its appearance at the cancerous, or critical period of life; it had occurred in a conſtitution ſubject to rheumatic complaints, the connection between which and future cancer is ſo well pointed out by the late Dr. Fothergill; and it had attacked more than one part of the ſyſtem, without any injury being urged as giving origin to it in this ſecond inſtance; it poſſeſſed alſo, every pathognomic appearance of cancer to the examination of the ſurgeon, and every feeling of it to the patient herſelf; nor to this hour would any doubt have even ariſen in Mr. Oliphant's mind reſpecting its nature, had it remained incurable. How trifling muſt his remark appear, which aſcribes its malignity to a cataplaſm of pigeon's dung, applied by the patient's friend! Such an application could have no bad effect, without a primary inherent cancerous tendency in itſelf.

From theſe remarks on the deſcription of the diſeaſe, I ſhall now examine the principle of cure, and on this principle no difference of opinion can exiſt. That a conſtitutional diſeaſe will at one time ſuſpend, and at another time entirely cure a local affection, is a fact which every tyro in medicine is acquainted with; but though acquainted with it as an acknowledged fact, much obſervation and experience are requiſite where this exertion of the powers of Nature is wiſhed to be imitated with ſucceſs. Indeed, the proper application of this circumſtance, leads to a ſucceſsful cure of moſt chronic diſeaſes; but in order to this ſucceſs, the means employed will require to be as varied as the nature of the diſeaſe, or chronic affection, which is to undergo the change. It is on this principle entirely, I conceive the cure of lues, or the conſtitutional venereal

venereal disease, to proceed: the action of mercury is properly nothing more than changing it from a chronic to an acute form, similar to small-pox and measles, or exciting and continuing a phlogistic action or pyrexia, till the expulsion of the disease. This opinion I suggested fifteen years ago, in a treatise on that subject; since that time I have found no reason to change my sentiments, in spite of the numerous chemical hypotheses and evanescent theories of the day. The operation of remedies, I am clear, can only be justly explained on principles consonant to the laws of the animal œconomy; and the subjecting an animated machine to the laws of inanimate matter, is losing sight of the power of vitality, and the peculiar energies of life, its leading distinctions.

Agreeing then fully with Mr. Oliphant in the general principle laid down, that the constitutional disease, in the present case, was the means of curing the local one, the only question betwixt us is, Whether this constitutional disease was the effect of accident or art? In order to make it the former, the same disingenuous attempt is shown on Mr. Oliphant's part, as in the description of the morbid appearances; to detect which, it is only necessary for me to detail the particular course I employed.

When I was first called in to see Mrs. Craib, the case appeared, as Mr. Oliphant justly observes, a hopeless one, and palliation was all the relief to be expected: The opinion I then gave, will mark, at least, the formidable nature of the malady, and prove, that Mr. Oliphant had no *favourable* doubts at that period. I prescribed for her a diet drink and pill; the former, with a view to produce some change in the state of the fluids; the latter, for the particular purpose of exciting the action of the lymphatic system: an opiate was also enjoined to be occasionally used, as indicated by the urgency of pain. To the sore itself a dressing of common cerate was applied, and the fermenting poultice laid aside at my desire, from the pain and irritation it constantly produces in every case of cancer. In this course the patient proceeded. Every day, according to her own account, she felt greater ease and abatement of pain, till the latter entirely ceased; and, as she expressed herself, she would not have known from her feelings any difference betwixt the ulcerated and the other breast. She could now lie with ease on the affected side, and use that arm with freedom; circumstances to which, previous to the commencement of this course, she was a stranger. The breast itself showed an evident shrinking of size; and Mr. Oliphant at that period observed, it was clear the progress of the disease was now *completely* checked. Reflecting on this favourable appearance, and desir-

ous

ous that it might proceed more speedily, I now suggested to Mr. Oliphant the propriety of employing frictions, so as to send the same remedies directly through the diseased part, without the circuitous course of the system at large. To this measure he objected, from the state of the weather and their supposed inefficacy; confident, however, in my own opinion, they were applied according to my direction by the person mentioned in his statement, as having formerly attended the patient as a friend. This practice was continued for the space of six weeks, in the course of which time, not only was the regular shrinking of the breast conspicuous both to that person and the patient, but several of the smaller tumours surrounding the diseased mass were entirely absorbed; facts, for the truth of which I appeal to themselves.—So completely satisfied was the patient of this, that, not content with the friction at night, as employed by her friend, she had recourse to it herself in the day, and thus it was carried beyond what was necessary for the actual cure of the disease. The subsequent pleuritic attack, therefore, may be considered in part, perhaps, as the consequence of her own imprudence, and it was attended with this farther mischief of interrupting the course too soon. During this time the scirrhous breast shrunk in proportion to the other, but never showed those strong marks of yielding softness till the course was at its height, at which period, even external symptoms of inflammation were conspicuous on its surface; and when examined some time after by the gentlemen who saw her, it was so soft as hardly to bear the name of scirrhous, and in a daily decreasing state. From this period, the management of the case, at Mr. Oliphant's special desire, was left entirely to himself; nor was I either consulted, nor, except from hearsay, knew what was employed. What I see of the practice, now sufficiently shows me the impropriety of the applications; but as it is his wish to claim all the merit of the case to himself, and for that purpose his statement was written, it is not my business to interfere with the subsequent treatment. From the detail of the prior course, it will be seen, that the progress of cure in this disease was gradual, regular, and bore a proportion to the activity of the means employed; that the relief experienced was from the commencement of this course, and that the constitutional disease was the consequence of the means employed, although aggravated by the imprudence of the patient herself, in a degree beyond what was necessary, and perhaps, also, by the accidental causes which Mr. Oliphant takes notice of: The termination, however, has been favourable, and would have been much more complete, but for the intervention of these causes. On the whole, neither the patient

tient herſelf, nor any perſon who ſaw her in her worſt, and apparently incurable ſtate, will agree with the propriety of Mr. Oliphant's quotation from Amatus Luſitanus, that it is better to let the diſeaſe alone. In making this quotation, he might have gone even a good deal higher, for he will find the ſame obſervation a trite adage from the time of Hippocrates downwards, though, if it has any truth in it, it applies more properly to the early than advanced ſtage of the affection.

After this view of the caſe, it remains that I ſhould examine the analogous caſes, cited by Mr. Oliphant, in ſupport of his ſtatement. All his caſes are evidently of a ſcrophulous nature; they were cured, the worſt of them, by bark and carbonic acid, medicines which will always, in real cancer, do harm. The only one that applies in any degree to that of Mrs. Craib, from its occupying the ſame ſituation, is the caſe of Mrs. Oſborn; but here the proceſs of cure, when examined, is materially different.

In Mrs. Oſborn's caſe, Nature, as it were, ſhrinking from itſelf, and unable to ſupport a load, allows it firſt to loſe its vitality, and then to ſeparate; and on this ſeparation ſuch predominant weakneſs prevails, that the veſſels are even unable to contract, or prevent, exceſſive hæmorrhages, which continue for a length of time. The ſkin alſo, in healing, becomes puckered in the ſame manner as in the healing of other ſcrophulous ulcers. Nor, during this proceſs, is the axillary ſwelling removed, but it continues for a vaſt length of time in a diſeaſed ſtate. Where, then, is the compariſon between this and Mrs. Craib's caſe? In the latter, a general increaſed action of the ſyſtem appeared; and from the moment of its appearance, the proceſs of abſorption was ſtrong and complete over the diſeaſe. No ſloughing or ſeparation took place, for the action of the abſorbents was equal to the removal; and the time in which this proceſs was accompliſhed, diſplayed the preſence of an active and daily increaſing cauſe, goading on Nature in her efforts, and threatening even injury to the ſyſtem, in order to its ſucceſs.

If inanition formed the principle of cure in ſcrophula and cancer, as contended for by Mr. Oliphant, it is evident theſe diſeaſes, among the lower claſſes at leaſt, ſhould be very readily cured; for Nature could never be at a loſs to have abundant opportunities of employing her exertions on the enfeebled, worn-out, conſtitutions of ſuch patients. But is there an inſtance of this ſort to be met with? Do not theſe diſeaſes make much more rapid progreſs amongſt the lower claſſes? And are not, alſo, the lower claſſes the moſt frequently attacked by them?

Before

Before concluding, I ſhall only beg leave to mention, that when the ſtatement of Mrs. Craib's caſe was to be made out, I mentioned the propriety of it to Mr. Oliphant, in which he readily acquieſced. When it was afterwards made out, it was read to him by me, and he ſuggeſted one alteration, which, at his deſire, I adopted; viz. that the cure was effected "without ſloughing by an increaſed abſorption, in which it differed from any he had ever ſeen." According to every idea I could then form of a man's ſentiments, the account drawn up by me, with this one alteration, had his unqualified approbation, and I even then told him it ſhould go in our joint names. On this conduct I ſhall make no comment; the matter is before the public. Three more caſes will be publiſhed by me in the courſe of a few weeks. Theſe will ſufficiently determine the point, and be the beſt anſwer to all the ſcepticiſm diſplayed on the ſubject.

In bringing Mrs. Craib's caſe into notice, it was my wiſh to call the attention of practitioners, in a particular manner, to this diſeaſe; and in doing ſo to ſhew,

1ſt. That the general principle of exciting phlegmonic action to a certain degree, and continuing it, is ſufficient to cure cancer, whether in the ſtate of ſcirrhus or ulceration.

2d. That the preferable mode of exciting this action is through the medium of the lymphatic ſyſtem.

3d. That the lymphatic ſyſtem, once excited, is capable of producing abſorption, equal to the removal of the moſt hardened maſs of diſeaſe.

4th. That the cure is to be ſought for in a proper ſelection of thoſe ſubſtances which are moſt powerful in exciting the lymphatic ſyſtem, and which continue this excitement in a permanent manner. I am,

Gentlemen,

Your obedient ſervant,

W. NISBETT.

St. James's Street,
Dec. 17, 1800.

To the Editors of the Medical and Phyſical Journal.

Gentlemen,

In a former communication I made ſome obſervations on the Rival Inoculations, and contended that the Vaccine had not ſo great a degree of ſuperiority over the Variolous, as on firſt reflection might be imagined; for that facts might be enume-

enumerated, to ſhew that inoculated Small-pox is very rarely contagious.

With your permiſſion I will now proſecute the inquiry according to promiſe. But, before I proceed to the facts which I intend to adduce, permit me to make a few preliminary remarks.

A part of the ſuperior merit of the Vaccine Inoculation, it is ſaid, conſiſts—in the diſeaſe induced not being infectious.

That the Vaccine Diſeaſe ſhould not infect the human ſpecies unleſs by contact, were I an advocate for the operations of contagions, I would account for; not becauſe it is unattended with ſuppurating puſtules, but from its being a diſeaſe incident to another animal.

That one inoculation ſhould produce infection, and the other not, from the circumſtance of ſuppurating puſtules merely, is not ſatisfactory, unleſs external ſuppuration be regarded as the *sine qua non* of contagion; which is not the caſe, or we ſhould no longer allow ſome other diſeaſes, as typhus, dyſentery, hooping cough, and the eryſipelatous ſore throat, to be contagious.

From the evidence which has been adduced, there cannot exiſt a doubt reſpecting the ſecurity which this analogous diſeaſe affords againſt the contagion of Small-pox. And it may be reaſonably ſuppoſed that miſtakes will every now and then occur, which cannot fail to injure the practice, or the profeſſional reputation of the practitioner. Then again, recent matter is more certain of producing infection than when it is dry; and fluid matter is, and probably always will be, more eaſily obtained by the generality of practitioners from Small-pox than from Cow-pox, by reaſon of the locality of the latter diſeaſe. And trifling as the operation of inoculation is, it is to be preſumed that many parents will diſlike thoſe repetitions ſo often neceſſary to communicate the Vaccine Diſeaſe.

Small-pox and Meaſles, though they are diſeaſes which affect the ſyſtem, differ from other univerſal diſeaſes, inaſmuch as they are independent of *excitement*. And it is this circumſtance which renders their *cauſes* leſs apparent than thoſe which induce ſthenic or aſthenic diſeaſes.

It would ſeem that Small-pox is not affected by impurity of air, for it rages with as great violence in villages as in cities.

Diſeaſes of the ſyſtem are preceded by prediſpoſition.—We know that prediſpoſition to general diſeaſe, is either a ſubtraction from, or an addition made to, that juſt degree of excitement which conſtitutes health. But with prediſpoſition to Small-pox we are unacquainted; it nevertheleſs exiſts, with ſome exceptions, throughout the ſpecies, at one time of life or other;

other; and it may, for aught we know to the contrary, mount up to diſeaſe frequently, appearing with little or no aſſiſtance from external aids.

That Small-pox is more under the dominion of prediſpoſition than dependant on the foreign powers, appears to be demonſtrated; for we know that many perſons who are obnoxious to the diſeaſe, are not affected by it when an epidemic conſtitution of the air is preſent, and when expoſed to the operation of the whole force of a ſuppoſed contagion; yet at ſome other period, when the ſtate of the air is unfavourable to this epidemic, and when they have had no communication with Small-pox patients for a great length of time, ſuch perſons become the ſubjects of it.

Prediſpoſition to Small-pox not being dependant on excitement, but of a nature peculiar to itſelf, can induce no other than a diſeaſe *ſui generis*; whilſt prediſpoſition, founded on a morbid degree of excitement, often terminates in diſeaſe different in form but not in eſſence. Thus we obſerve, that a diminution of excitement exiſting amongſt perſons of the ſame community, on their being expoſed to the application of a ſimilar foreign hurtful power, ſhall give occaſion to typhus, dyſentery, or ſome other diſeaſe of the aſthenic claſs. But I ſee no reaſon that contagion ſhould be reſorted to, to account for thoſe ſpecific diſeaſes originating in a *prediſpoſition* not founded on excitement, more than there is to refer to thoſe unknown undefined *ſomethings* to account for *plague, typhus*, or *dyſentery*; diſeaſes which are known to be dependant on the *effect* produced by the exciting powers, namely the excitement. And I am of opinion it makes not for Dr. Maclean's arguments to prove, that the *plague never ariſes from contagion*,* his ſuppoſition that Small-pox and Meaſles are the *effects* of ſpecific contagions from the invariableneſs of the diſeaſes induced, and from the conſideration that man is incident to theſe only once during life.

Whether man be incident to ſome diſeaſes which affect the ſyſtem only once during life, or to others which may affect it more than once, I preſume makes no difference, ſo far as relates to *contagions* as *cauſes*. The circumſtance adds nothing to the evidence that ſuch *powers* have no exiſtence; nor has it the leaſt tendency to prove, that any ſpecific contagion capable of inducing a diſeaſe *ſui generis* through the medium of the atmoſphere really exiſts.

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* A Diſſertation on the Source of Epidemic and Peſtilential Diſeaſes, by Charles Maclean, M. D.

Effects are often aſcribed to *cauſes* which have no exiſtence, or, if really exiſting, inadequate often to the production of thoſe *effects*; and it may frequently happen that Small-pox, inſtead of being attributed to *contagion*, might, with greater propriety, be referred to *a variolous epidemic conſtitution of the air*, operating with prediſpoſition ſo as to induce the diſeaſe.

Again;—In conſequence of the mortality occaſioned by caſual Small-pox, as is apparent from the London Bills having been conſiderably augmented ſince the introduction of inoculation; from a ſeeming coincidence of circumſtances, by no means regular, as will appear on inquiry, has this increaſe been aſſigned to a *cauſe* ſufficiently plauſible, as flattering popular prejudice,—*to the practice of inoculation.* Thus has the practice not had fair play; and by means like theſe, have envy, hatred, and malice been excited, not only between the poor and their rich neighbours, but between perſons reſident in the ſame houſe.

It will appear, from the extracts which I ſhall have occaſion to make from a valuable work, that the increaſe of mortality from Small-pox, commenced long before the introduction of inoculation; and, that it continued to increaſe by a regular progreſſion, until, from the prevalence of the practice, a decreaſe became obſervable. Although a prediſpoſition not founded on *excitement*, is of a nature unknown to us, yet are we obliged to admit its exiſtence, for the reaſons which have been aſſigned; and by this admiſſion we extricate ourſelves from thoſe difficulties in which contagions involve us.

Prediſpoſition to Small-pox, is a ſtate of the body not acquired from reſidence in any particular climate. Man, whereever placed, is endowed with it: the heat of Africa, and the cold of Ruſſia, are alike favourable to it.

The univerſality of Small-Pox, and the regularity of its recurrence, are circumſtances, I think, unfavourable to the idea of its being propagated by *contagion.*

If a diſeaſe is dependant on *contagion* for its continuance, if the *contagion* be loſt, the diſeaſe would neceſſarily diſappear. It may poſſibly be obſerved, that the Variolous Contagion, from having extended its influence over the earth's whole ſurface, by taking poſſeſſion of every *pillow* and *bolſter*, *chair* and *table*, and every thing elſe, moveable and immoveable, cannot be deſtroyed either by accident or deſign; yet, one ſhould have expected, that ſome given degree of heat would have rendered it inert.

If Inoculated Small-pox be not infectious, how comes it that matter taken from inoculated patients conveys the diſtemper with equal certainty as if it was taken from the natural Small-pox?

pox? has been asked. Is it not morally certain, say they, that the *effluvia* partake of the same infectious quality?

To this moral certainty, I beg leave to reply, that *effluvia*, in some other instances, we know, will not induce disease; but if the matter of the effluvia be absorbed, the disease is occasioned.

Surgeons and nurses may by accident inoculate themselves with syphilis, in places appropriated for the reception of venereal patients; but the air of the hospital, which is strongly impregnated with effluvia arising from the respiration, the ulcers, and the abscesses of the diseased, is inadequate to produce such an effect.

Practical anatomists respire an air loaded with putrid effluvia for many hours together, with impunity; whilst a small quantity of this fluid matter, if absorbed, sometimes gives occasion to a disease which has proved fatal.

Facts are stubborn things; they make an impression on the mind when arguments fail. That I may not incur the displeasure of any of your readers, I will, without further preface, relate such as will serve to vindicate a practice which is threatened with disuse; and to which we are under great obligations, from a charge which appears not to be well substantiated; and for these I am indebted to Dr. Watkinson.*

"Medicus, a very eminent and experienced German physician, observes, in one of his epistles to Dr. Petit, of Paris, that the Variolous Contagion is so rarely propagated by the artificial disease, that, although a prodigious number of people have been inoculated, not more than *ten instances*, perhaps, can be reckoned, in which it has communicated the infection, notwithstanding the pains which have been taken to discover that pretended quality of it."

"Miege, a celebrated inoculator, declares, that his own experience has afforded but one example of the contagion being propagated by the inoculated Small-pox, and that, that happened by contact, *per osculum, ideoque proximum per contactum accidit.*"

"Sulzer, who appears to have had considerable practice in this art, assures Professor Schroëder, of Gottingen, in a letter which he wrote to him in the year 1765, that he had not seen *a single case in which the inoculated Small-pox had by contagion given the disease to another.*"

"A simi-

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* An Examination of a Charge brought against Inoculation, by De Haen, Rast, Dimsdale, and other writers; by John Watkinson, M. D. Printed in 1777, for J. Johnson, London.

"A ſimilar aſſurance is given by Dr. Odier, of Geneva, to the author of the *Journal de Medecine*."*

"Mr. Holwell, who reſided upwards of thirty years in the Eaſt Indies, and whoſe account of the manner of inoculating the Small-pox in that country, ſhews the attention that he paid to this ſubject, informs us, that "notwithſtanding the multitudes that are every year inoculated there in the uſual ſeaſon, it adds no malignity to the diſeaſe taken in the natural way, nor *ſpreads the infection as is commonly imagined in Europe*."†

But the following fact, atteſted by Dr. Schwenke, a phyſician of diſtinguiſhed reputation in Holland, is ſufficient, I think, to remove every doubt that may remain on this head:

"About the end of the year 1767, and the beginning of the year 1768, two hundred people at leaſt were inoculated at the Hague, who, without much regard either to themſelves or others, frequented all places of public reſort, notwithſtanding which, no epidemic was produced, nor in the whole year did more than eight perſons die of the Small-pox; and of theſe, three died in the ſpring, one by inoculation, and two by the natural diſeaſe, which they had caught at ſome other place and carried with them to the Hague; the remaining five died towards the end of the year. *Vid. M. W. Schwenke Epiſt. in Cel. Sandifort. Biblioth. Med. Tom. 6.*"

[The remainder of this Communication is obliged to be poſtponed at preſent for want of room.]

To the Editors of the Medical and Phyſical Journal.

Miſtaque cum veris paſſim commenta vagantur
Milia rumorum: confuſaque verba volutant.

GENTLEMEN,

THE rumours concerning the diſcreditable caſes of the inoculated Cow-pock lately at Clapham, were ſuch as to excite the attention, and provoke the inquiry, of perſons even little intereſted in the fortunes of the new practice: But from the part I have taken in attempting to introduce the Vaccine Inoculation, I found myſelf particularly called upon to relate ſome

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* Journal de Medecine, &c. Vol. XLII.

† An Account of the Manner of inoculating for the Small-pox in the Eaſt Indies.

ſome particulars, and to aſſign reaſons for thoſe unfavourable occurrences. I confeſs I was much diſappointed to find that the Report publiſhed in your laſt Journal, not only did not ſatisfy my mind with reſpect to the aſſigned cauſe of theſe unfavourable inſtances, but contained ſeveral aſſertions of great practical importance, which are at variance with the evidence of my own experience. Hence I thought it would be culpable conduct on my part, to withhold from the Public the adverſe evidence which I poſſeſs, ſincerely hoping that the communication of it will be beneficial, whether the concluſions in the Report be invalidated or not.

The following paragraph contains the moſt important aſſertions, which principally induced me to come forward on the preſent occaſion. "The gentleman," I am authorized to ſay, Mr. Buckland, "who performed this inoculation, candidly acknowledges, that the matter with which he had charged the lancet for this purpoſe, had been taken at a very late period of the diſeaſe, after a brown ſcab had formed, and had a purulent appearance at the time it was taken from the arm, conſequently had, by degenerating, loſt the power of giving the Cow-pox infection, and appears to have been the cauſe of all the miſchief that enſued."

Now, as the concluſions in the Report are principally built upon the kind of *Matter* employed, it is to my apprehenſion needful to aſcertain with preciſion, the origin, and many of the properties and circumſtances of this inoculated *Matter*, to which ſo much miſchief is imputed. Accordingly I find, on the teſtimony of the Inoculator,

1. That it was taken on the lancet, uſed in the firſt inoculation mentioned in the Report, from two of the patients of that inoculation on *the tenth day*, and who underwent the diſeaſe favourably. This lancet had not been cleaned ſubſequently to the firſt inoculation, but it was not ruſty, and the matter on it was inſerted while fluid, within an hour after it was taken; and no other matter was taken on this lancet employed in the inoculation, but that from the two patients, as aſſerted moſt diſtinctly and uniformly to me by Mr. Buckland in three different converſations.

2. The matter uſed in the firſt inoculation, may be ſafely aſſumed to have been that of the genuine Vaccine pock, from the words of the Report, namely, "that there were no appearances in any degree deviating from the progreſs of the Cow-pox." It is however, I think, material to remark, that Mr. B.'s ſtatement informs me that one of the patients of the firſt inoculation had unfavourable ſymptoms, like thoſe of the ſecond inoculation, and that in ſeveral of the patients unfavourable ſymp-

toms

toms supervened soon after taking matter with the lancet on the tenth day, for the second inoculation.

3. It is next in order in this investigation, to inquire into the source of the matter inserted in the first inoculation, which was performed on Wednesday, October 22. Here we find that Mr. B. collected matter on a *lancet*, from a patient at Clapham, under the care of Mr. L.* an eminent Practitioner in the city, on *the twelfth day* after inoculation; the patient having been inoculated on Saturday, October 11, and it was employed while fluid, within an hour after it had been obtained, and the Report states, on "fifteen persons, but only six took the infection."

4. It is of importance to be known, that Mr. L. above alluded to, on Thursday, October 23, *the thirteenth day* after inoculation, a day later than Mr. B. "finding," says he, "the inflammation round the pustule still continuing bright, and *the fluid very thin*, I took a little on the point of a lancet for Mr. D. which being scarcely visible, he doubted of success; with this he inoculated a child on Saturday morning, (November 1): The inoculation succeeded, and numbers have been since inoculated from this source, and one is under my care at this time, the eighth day." This last-named patient was obligingly shewn to me by Mr. L. on Saturday, December 13, then the thirteenth day after inoculation, having the usual appearances of the vesicle in the incipient state of desiccation.

I am under the necessity of delivering evidence from my own observation, which contravenes the conclusion drawn in the Report; that as matter was taken late in the disease, and after a brown scab had formed, which had a purulent appearance, it "*consequently had, by degenerating, lost the power of giving the Cow-pock infection.*" Now my own practice, as well as that of some of my friends, instructs me, that purulent matter formed in a genuine Vaccine pock, not later than the 12th day after inoculation, can produce the genuine mild *Vaccina*. If analogy were needful to establish this truth, I would appeal to the practice of the variolous inoculation; in which, I believe, no Inoculator hesitates to use purulent, as well as limpid, matter. However, I do not mean to say, that it is proved that purulent vaccine fluid is equally efficacious as the limpid fluid; for suppuration so seldom takes place, that I have not facts to prove more than the harmlessness and efficacy of such matter where I have used it; but consequently, that purulency does not necessarily imply, as asserted, *degeneration*.

To

* This patient of Mr. L. was inoculated with matter of the tenth day, from Mr. L——'s, the surgeon, child.

To prove that the *Matter* uſed by Mr. B. was the occaſion of the anomalous vaccine caſes, it is ſtated in the Report, "that in two of the patients who are believed to have had the Cow-pock diſtinctly by the former inoculation, the ſame ſymptoms were unfortunately induced in attempting to procure Cow-pox fluid from them." If the lancet was in the condition I have above ſtated, I preſume to think that the enſuing inflammation, at leaſt partly, aroſe from the inciſions, or would have taken place, as is frequently the caſe, when the veſicle begins to dry, independent of any external irritation. Nor is this opinion diſcountenanced, but rather confirmed, by the *great proportion* of ſimilar anomalous caſes in the late inoculation; for examples are wanting of a particular morbific matter, which will produce on healthy parts the effects on the places inoculated ſo generally as related in the Report.*

Another fact, aſſerted on good authority, inclines me to the opinion, that if venomous matter, different from the vaccine, were propagated, the vaccine was alſo mixed with it, is, that in one or more of the caſes a genuine vaccine veſicle exiſted on one arm, while on the other arm of the ſame patient a conſiderable ulceration took place.

With reſpect to the five injunctions, as collected from a reſpectable authority, in the Poſtſcript of the Report, I ſhall only take notice of two of them:

1. "That the Cow-pox *fluid* ſhould be taken not later than the *ninth* day of the diſeaſe." This injunction was delivered, I believe, when the evidence of caſes of inoculated Cow-pock was extremely ſcanty, comparatively with the ſtate of it at this time, certainly in the proportion of 1 to 100. Among the great number of caſes which have fallen under my own obſervation, and from the ſtill greater number which have been communicated to me, not a ſingle unfavourable caſe, on the account of the matter being taken at any period between the ſeventh and thirteenth day, or even, in ſome inſtances, a day or two later, has been obſerved. And it cannot eſcape notice, that the caſes referred to in this paper, are evidences againſt the injunction. The Regiſters at the Vaccine Inſtitution, which ſeem adequate to the determination of this point, ſhew no difference in the degree or kind of ſymptoms of the inoculated *Vaccina*,

* As to the inflammation of the punctured part and conſtitutional diſorder which occurs ſometimes in diſſection, ſuch a caſe does not happen, I believe, in more than one out of fifty or perhaps an hundred inſtances of puncture; and on the very firſt authority I am aſſured, it ſeems that the morbid effects ariſe ſometimes from the punctures even with a clean needle or knife, independent of animal matter.

whether the matter was of the eighth or of the eleventh and twelfth days; ſo that the Inoculators, as far as I know, give no preference. However, to ſatisfy my own mind, in the opportunities which occurred after reading the Report, I made ſome trials with preciſion, by inoculating one arm of each patient with *Matter* of the ſeventh or eighth day, and the other with *Matter* of the twelfth day; but I could obſerve no difference in the appearances of the Vaccine pock on each arm of the ſame perſon. Among theſe caſes were two young children, who were inoculated, under my direction, by Mr. Donelly; and to prove that the matter was equally efficacious, I ſhall ſtate, that with matter, on the eighth day from theſe patients, taken on twenty-ſix lancets, and in the glaſs bottles* with ſpoon ſtoppers, invented by Mr. Lewis, the apothecary to the Vaccine Inſtitution, Mr. Carpue ſuperintended the inoculation of forty-two perſons in the army; of whom, twenty-five took the diſeaſe, which they underwent with the uſual mild ſymptoms, and without any perceivable difference in the ſymptoms depending on the age of the matter.

With reſpect to the ſecond injunction, I have already had occaſion to deliver the teſtimony of my experience.

Whether the preceding evidence be ſufficient to make out a Caſe manifeſting that the anomalous and ſevere ſymptoms *did not depend on the Matter uſed having been taken ſo late as the tenth day, from under a ſcab, and allowing that it was purulent*, I ſubmit to the deciſion of the medical public: Nor is it within my preſent deſign to bring into view other agents, which might occaſion the bad ſucceſs. I ſhall only juſt obſerve if that had been my deſign, I ſhould particularly have pointed out the probable conſequences of inoculating ſo many perſons with *Matter on one lancet*. At the ſame time it would have been but juſtice, to confirm Mr. Buckland's depoſition, by the extreme improbability of his taking *Matter* from any other ſource but his own patients, on the tenth day. As to the gentlemen who have written the Report, I think it would be injuſtice to their reſpectable characters, to ſuppoſe an apology neceſſary, where *the manifeſtation of truth is the object*.

For the ſatisfaction of ſome perſons, it may be needful to obſerve, that the preſent queſtions ought not to be allowed the weight of a feather in argument againſt the ſafety and efficacy of the Vaccine Inoculation, although they are extremely important for determining on what the ſucceſs depends, for avoiding hurtful agents, and for advancing knowledge of the animal œconomy. Nor will I, on any deciſion of the points at iſſue,

* Theſe bottles may be had at Meſſrs. Parkers'.

iſſue, recall an aſſertion which I have already made, that "a perſon during the inoculated Vaccina has a greater chance of life than under the ordinary circumſtances of living." But this propoſition is ſuppoſed only to be true, provided; 1. The Inoculator poſſeſs the knowledge already attained. 2. That he apply that knowledge with ſkill and ſedulous attention. 3. That he have the command over, or can avoid, the agency of thoſe powers which experience has ſhewn to be hurtful. 4. That the patient be obedient to the directions preſcribed. Hence, however, it is manifeſt, that a patient may have a dangerous diſorder, and even die under the inoculated Cow-pock, and yet the Practitioner be blameleſs. Although I very much doubt whether any perſon has ever died of the inoculated Vaccina, the above requiſites being preſent, it would be unwarrantable to affirm, that death may not take place either from the Cow-pock or from intervening diſorders during the Cow-pock; for Phyſic, even in its preſent improved ſtate, cannot always "hold Mortality's ſtrong hand;"—it does not yet "bear the ſheers of Deſtiny."

As to one particular objection to the new Inoculation, on the ſcore of the origin of the venom in the Cow, and the offenſive aſpect of the eruption; if *uſefulneſs* be here the object of contemplation, a gem will be ſeen under a deformed exterior,

> "Which, like the toad, ugly and venomous,
> Wears yet a precious jewel in his head."

I am, yours, &c.

GEORGE PEARSON.

Leiceſter Square, Dec. 24, 1800.

P. S. In a Note to the Report under examination, the change which *Matter* undergoes in the Vaccine and Variolous puſtules, when it becomes virulent, it termed *decompoſition.* If this term had not an allowed definite import, and would not be likely, from the authority of the writer, to be employed in an unmeaning, vague ſenſe; or, what is worſe, according to its import; it would not be worth while to notice the impropriety of its uſe in this place. This impropriety will be moſt ſpeedily perceived by the queſtion, *What is the kind of change which takes place?* Now, I anſwer, without fear of contradiction, in what the change conſiſts is utterly unknown; but that as it is quite as reaſonable to *imagine* that it conſiſts in a new *compoſition* as in *decompoſition*, I beg to uſe the term *change* or *alteration*, and diſcard the term *decompoſition*, leſt a thing unknown, by the imagination bodied forth, obtain "a local habitation and a *name*."

CRITICAL RETROSPECT

OF

MEDICAL AND PHYSICAL LITERATURE.

An Essay on Phlegmatia Dolens, including an Account of the Symptoms, Causes, and Cure of Peritonitis Puerperalis et Conjunctiva, &c. &c. By JOHN HULL, M.D. 8vo. 370 pp. 6s. 6d. London. Bickerstaff.

WE can recommend this work as g... a very full and elaborate history and description of a disease incid... to lying-in women, and commonly known by the name of *œdema lacte...m*, or white leg:—The subject is treated of in the following or...r:

"1. The *literary history* of the disease is given, together with a *review of the different theories* that have been formed concerning it.

"2. The *medical history* is given, and the *causes* and *cure* considered.

"3. The *analogy* of this disease with *Puerperal Fever* (Peritonitis *puerperalis* Ill. Frank.) and *Rheumatism* is pointed out.

"4. The *characters* and *synonyms* of the genus Phlegmatia with its species are presented, and the proper place assigned to it in the nosological system of the late Dr. Cullen."

In the literary history, which occupies 130 pages, the author has given copious extracts, and has frequently copied whole cases from every writer since the days of Hippocrates, who has mentioned this complaint; and we should be inclined to bestow some praise on this part of the work, as an useful epitome for those who wish to be informed of what has been written on the subject, if it was not sullied with a degree of asperity, and an unnecessary attack upon Dr. Ferriar, a gentleman of acknowledged eminence in his profession, and whose talents, as a man of letters, are of the highest order.

It is painful and vexatious to observe this contentious disposition so frequently breaking the bounds of moderation and urbanity.

"*Rest—rest, perturbed Spirit.*"

The author has illustrated the medical history of the disease by several cases, and amongst the exciting causes he enumerates, "1st. Contusions, or violent exertions of the lower portions of the abdominal and other muscles inserted into the thighs and pelvis, and of the muscles of the inferior extremities and contusions of the cellular texture connected with these muscles, during a tedious labour.—2dly. The application of cold and moisture.—3dly. Suppression or diminution of the lochia, and of the secretion of milk. 4thly. Food taken in too large quantity and of a too stimulating quality, especially when the patient does not give suck.—5thly. Standing or walking too much before the arteries and veins of the lower half of the body have recovered sufficiently from the effects of

of the diſtention which exiſted during the laſt months of pregnancy."—The author believes the proximate cauſe " conſiſts in an inflammatory affection, producing ſuddenly a conſiderable effuſion of ſerum and coagulating lymph from the exhalants into the cellular membrane of the limb."

" I am induced to offer, by way of recapitulation, the following conciſe account of my notion of the formation, eſſence, &c. of Phlegmatia *dolens:* A patient, prediſpoſed to the diſeaſe, being ſubjected to the action of ſome, or all of the exciting cauſes abovementioned, the balance of the circulation is changed, and an unuſual determination of blood to the lower part of the trunk, or to one or both of the inferior extremities, taking place. Hence the arteries of the part more particularly affected, are ſtimulated to increaſed action; inflammation is produced, and a conſiderable quantity of ſerum and coagulating lymph is effuſed from the exhalants into the cellular membrane, which moderates the inflammation and prevents it from extending to the outer ſurface of the cutis. The inflammation and ſwelling are ſometimes more limited, but, in general, they (ſooner or later in different patients) are diffuſed over the whole of one limb, and not unfrequently the other lower extremity in a few days becomes affected in a ſimilar way.—Whilſt theſe appearances are obſerved in the limb, the whole ſyſtem exhibits ſymptoms of conſiderable pyrexia; and theſe febrile ſymptoms, in many inſtances, precede the topical affection. After ſome time, the ſymptoms of pyrexia and of topical inflammation ſubſiding, the general health of the patient begins to improve, and the ſwelling of the limb, inſtead of being tenſe, elaſtic, hot, exquiſitely tender and painful when touched or moved, as before, becomes of a natural heat, is ſtiff and numb rather than painful or tender, yields more eaſily to the finger, and retains the impreſſion for a conſiderable time; in ſhort, the intumeſcence now approaches more and more to Anaſarca or Œdema, and, for the moſt part, gradually diſappears. The diſeaſe, occaſionally, has a different termination, as has already been related.

" The œdematoſe ſtate of the limb towards the decline of the complaint may be eaſily accounted for, upon the ſuppoſition that the power and action of the abſorbents is totally unaffected; for, after the exhalants have been ſo far over-ſtretched as to pour out ſerum and coagulating lymph, we may naturally expect, that they will be a conſiderable time before they recover their tone, and be as much contracted as in their natural ſtate; conſequently, till that period arrive, a larger quantity of ſeroſity, eſpecially when the patient is in an erect poſition, will be effuſed from them into the cellular membrane, and accumulated in the limb, unleſs we prevent it by bandaging the limb, or increaſing the action of the abſorbents. From this view of the diſeaſe it appears, that the whole of the phenomena may be ſatisfactorily accounted for independently of any primary affection of the lymphatic ſyſtem of the limb. Whilſt, on the other hand, they cannot be explained independently on an original affection of the ſanguiferous ſyſtem.

" SECTION

"SECTION THIRD.

" *Of the Cure of Phlegmatia Dolens.*

" In this complaint there are three periods, which require different modes of treatment, and which are more or leſs diſtinctly marked, both with reſpect to the ſyſtematic and topical affection, in different caſes. Hence, in delivering the method of cure, it becomes neceſſary to advert to theſe three ſtages.

" In the firſt, which may be ſtyled the *inflammatory phlogiſtic*, or *ſthenic period*, the affection of the ſyſtem, for the moſt part, conſiſts in an inflammatory diatheſis, or increaſed tone and action of the heart and arteries, both with reſpect to frequency and force; and the affection of the limb conſiſts in a ſtill more violent action of the arteries diſtributed to it, producing effuſion and an extreme degree of tenſion of the ſkin.

" For the removal of the ſyſtematic affection, the indications are,—

" 1ſt. To diminiſh the quantity of the circulating fluids.

" 2dly. To remove, or moderate thoſe irritations, to which the body is almoſt conſtantly expoſed.

" 3dly. To employ ſedatives.

" 4thly. To employ remedies which determine to the ſurface.

" The 1ſt indication is fulfilled by blood-letting, purgatives, and emetics.—Emetics are uſeful alſo as revulſives, and by determining to the ſurface.

" The 2d indication is fulfilled by avoiding exerciſe of the mental faculties; by keeping the body quiet, and conſtantly in an horizontal poſition; by uſing a low vegetable diet, and diluting acidulated liquids; by keeping the bedchamber cool, and avoiding too great a quantity of bed-cloaths, and by keeping the bowels gently open.

" The 3d indication is ſatisfied by employing remedies, which directly diminiſh the action of the heart and arteries, as digitalis, acids, &c.

" The 4th indication is anſwered by employing neutral ſalts, antimonial preparations and ipecacuanha, either ſingly or conjointly; and ſtill more completely, by combining opium with one or more of theſe remedies, and by the uſe of the warm bath.

" For moderating the violent action of the arteries of the parts affected, local blood-letting by means of leeches, and bliſters or rubefacients, are to be employed. For relieving the tenſion of the ſkin, emollient fomentations, liniments, and ointments, are to be applied: punctures, or inciſions, though ſometimes employed with this view, are rarely very efficacious, on account of the matter coagulating very ſoon after its effuſion.

" § 2.

" In the third, which may be named the *antiphlogiſtic*, or *aſthenic period*, the ſyſtematic affection conſiſts in univerſal debility, in a diminution of the tone and action of the ſanguiferous ſyſtem, and in

in an impoverished state of the blood.—The local affection appears to consist in a relaxation of and increased effusion from the exhalants of the limb; and it is not improbable, that the action of the absorbents is sometimes diminished.

"For the cure of the affection of the system, the indications are:—

"1st. To increase the general strength and the tone and action of the heart and arteries by bitters, tonics, stimulants, and exercise, especially in a carriage.

"2dly. To improve the state of the blood by a nutritious diet, adapted to the powers of the digestive organs.

"For removing the topical affection, the indications are,—

"1st. To lessen the effusion of fluid from the exhalants; which may be effected partly by the remedies used for the cure of the systematic affection, and partly by the application of a tight bandage, by the cold bath, or cold water, fresh or salt, dashed upon the limb; and by avoiding too much standing, or walking.

"2dly. To increase the action of the absorbents of the limb; which is to be done by the application of heat, blisters, electricity, and other stimulants; by friction with the hand, or a flesh-brush; by digitalis, squill, mercury, carbonates of soda, potash, and ammoniac, &c.

"It is not sufficient that the absorbents take up what is daily effused by the exhalants in this stage of the complaint; this will merely render the swelling stationary. It is necessary that they absorb considerably more, in order that the lymph and serum, accumulated during the inflammatory stage, may be completely removed, and the limb return to its natural size.

"§ 3.

"In the SECOND, or *intermediate* PERIOD, the systematic affection is to be treated by the remedies pointed out as fulfilling the 2d, 3d, and 4th indications in the 1st stage: But these are not to be employed so rigorously.—The topical affection is to be remedied by gently stimulating liniments, ointments, &c."

Before we proceed to the rest of the work, we take this opportunity of mentioning an application for this disease, which has been used with invariable success, for several years, at one of the best regulated lying-in hospitals in London, and where our readers will readily suppose, from the great number of women annually delivered there, that instances of the *œdema lacteum*, or *phlegmatia dolens*, are not very uncommon. It is the practice at that hospital, to apply flannel well soaked in hot vinegar to the groin of the affected limb, and to renew it frequently; and we are well assured that no other remedies beyond those necessary to keep the bowels regular are ever used, notwithstanding the fever, pain, &c. which commonly accompany this disease.—The author next points out the connection and analogy subsisting betwixt *phleg. dolens* and *peritonitis puerperalis* in their histories, predisposing causes, exciting and proximate causes, and their methods of cure. Of the symptoms of the latter disease he has given a good description, and

and the following paſſage will give the author's opinion on its infectious nature, and identity with *Typhus*, neither of which he believes it is allied to.

"As far as my obſervation goes, Peritonitis *puerperalis* is not infectious. I have never ſeen a caſe, wherein I had reaſon to ſuppoſe, that the effluvia ariſing from the patient, produced puerperal fever, or typhus, or any other diſeaſe in another perſon, either directly or indirectly. The diſeaſe in queſtion frequently ariſes, where there is not the leaſt foundation for a ſuſpicion that infection has been applied. And I have, in more than one inſtance, delivered a patient in an advanced ſtage of typhus, without any ſymptoms ſupervening, that indicated an inflammatory affection of the peritonæum."

In the method of cure, the reader will perceive that no ſpecific plan is recommended, but the remedies are adapted to each particular ſpecies and habit.

"Some practitioners are, I believe, of opinion, that every caſe of a particular diſeaſe ought to be treated in an uniform manner, and hence they do not give themſelves the trouble of adapting remedies ſpecially to the different caſes that occur. Of this claſs, ſome probably may have excluſively adopted M. Doulcet's, and others Dr. Gordon's method of treating puerperal fever, according as they happened to become acquainted with one before the other, or to have an averſion to, or predilection for, the uſe of the lancet. If this be the caſe, theſe practitioners would do an eſſential ſervice to the public, by giving a full account of their ſucceſs. If this were done, I ſuſpect, that from the alleged facts it might be proved, that neither plan ought to be indiſcriminately adopted to the excluſion of the other.

"From what I have ſeen of this complaint, I am of opinion, 1ſt, that an emetic, adminiſtered early, will cure the diſeaſe in many caſes; 2dly, that general blood-letting, and briſk purging, may, in other caſes, be employed freely with ſucceſs; 3dly, that theſe two methods may, in ſome inſtances, be advantageouſly combined; 4thly, that in ſome caſes the diſeaſe may be cured without employing either general blood-letting, emetics, or briſk cathartics; 5thly, that other remedies, which have been neglected as uſeleſs and unneceſſary, or condemned as hurtful, may be joined with Doulcet's and Gordon's plan with very great advantage.

"With this impreſſion on my mind, I enter upon the conſideration of the cure of puerperal fever, although the plan I have to recommend is neither ſimple, nor uniform, nor nearly approaching to infallibility.

"The indications of cure, which ariſe in puerperal fever, reſpect either the ſyſtematic or abdominal affection. Thoſe which belong to the former head, are,

"1ſt. To moderate the action of the heart and arteries.

"2dly. To ſupport the action of the heart and arteries.

"3dly. To prevent, or correct, the putreſcency of the fluids.

"4thly. To palliate, or remove, urgent ſymptoms.

"Thoſe

"Those which relate to the latter head are,

"5thly. To moderate and subdue the increased action of the peritoneal blood-vessels, and thereby prevent the mischief arising from extensive adhesions, from the effusion of fluids into the abdomen, from suppuration and gangrene.

"6thly. To promote the absorption of the effused fluids.

"7thly. To favour the discharge of pus when formed."

The last chapter treats on the character, synonyms, description, and diagnosis of the *genus phlegmatia* and its species, and is a specimen of the nosological abilities of the author.

MONTHLY REPORT of DISEASES,

Admitted under the Care of the PHYSICIANS of the FINSBURY DISPENSARY, St. John's Square, Clerkenwell.

From November 20 to December 20, 1800.

	No. of Cases.		No. of Cases.
Continued Fever	49	Hypochondriasis	2
Small-Pox	5	Dyspepsia	8
Erysipelas	2	Gastrodynia and Enterodynia	8
Cynanche Tonsillarum	3	Hysteria	4
Pneumonia	4	Epilepsy	3
Hæmoptysis	2	Vertigo	2
Phthisis Pulmonalis	9	Cephalæa	5
Cough and Dyspnœa	44	Hemiplegia	2
Catarrh	8	Dropsy	9
Dysentery	1	Asthenia	25
Diarrhœa	4	Diseases of Infants	14
Chlorosis and Amenorrhœa	10	Chronic Eruptions	10
Menorrhagia	6		

From the above list it will appear, that fevers of the typhoid character continue still to prevail, with scarcely diminished violence, among the lower classes in this metropolis.

For a very long period there has never occurred a season that has been characterized by such an universality and malignity of febrile contagion.

Many of the probable causes of this peculiar prevalence of contagion have been stated in more than one of our Reports; unfortunately, a principal of these causes, the want of food, is likely to grow still more fatal and extensive in its operation: what the circumstances are which may have induced this want, it is far out of the province of a Medical Writer to inquire; but it is within his province, and even it is his indispensable duty, to make known facts that are of the most pressing importance, and that almost exclusively fall within the sphere of his professional observation. It is a fact, and a most miserable fact it is, that a large proportion of the disease with which the poor in London are afflicted, arises from a deficient quantity of natural and wholesome nourishment. To such feeble, hungry, and emaciated wretches, the administration

tion of drugs is farcical; it may be even regarded as cruel and inſulting: to thoſe who are aſking for bread, it is giving a ſtone. Not long ſince, the Author of this Report was called to a family who, he was told, had been attacked by fever; but after examining the ſkin, the tongue, the pulſe, the countenance, and the late hiſtory of his poor patients, he ſoon found that their pitiable condition was entirely to be attributed to their not having taken food for three days. He beheld a child with his eyes open; but, as far as it could be aſcertained from appearance, altogether inſenſible. Not the fainteſt veſtige of ſenſation, or power of motion, remained. His lower limbs had been for ſome time in a ſtate of contraction, but it was remarkable that this contraction was removed by expoſing them for a few moments to the vivifying influence of the fire. His mother lying beſide him on the ſame miſerable bed, although from a ſimilar cauſe herſelf unable to ſpeak or move, every other moment caſt a melancholy and anxious glance on the countenance of her famiſhed and expiring child. The writer has been particularly induced to enlarge more perhaps than is conſiſtent with the proper bounds, and the general intention of this article, with regard to the preſent afflicting ſtate of the diſeaſed poor, as a plan for their relief he underſtands is at preſent in actual contemplation.

The nature of houſes of recovery has been explained, and the urgency of their importance has been already enforced in one of our medical reports. From that high ſenſe of generoſity, for which the inhabitants of this great capital are celebrated, almoſt as much as for commercial induſtry, it is beyond all doubt that they will be able and willing to carry into ſpeedy and extenſive effect, an inſtitution that will conduce more eſſentially to the good of the lower claſſes of the community, and will redound more to the real honour of the higher, than perhaps any medical eſtabliſhment that has as yet been formed, or which even in future is likely to be conceived.

It is not amiable, nor is it reaſonable to believe, that mankind are ſo inſenſible as ſometimes they are repreſented to be; if they do not alleviate diſtreſs, in general it is becauſe they do not *ſee* it.

Let a perſon, however little diſtinguiſhed he may be for his kindneſs or humanity, accompany a phyſician in his melancholy round, among the ſick poor, and he will feel his heart yearn for their miſery, and will find his hand open for their relief.

It is neceſſary to remark, that in conſequence of wanting the ſupport of wholeſome food, and the other unhappy circumſtances of their condition, the poor in London are too frequently induced to ſeek a temporary relief from the ſtimulating operation of the worſt of ſpirits. * Seldom poſſeſſing reſolution of mind enough for the

* Such reflections as theſe, awakened by late ſubjects of actual obſervation, may recall perhaps to the memory of the literary reader, ſome melancholy paſſages in the Hiſtory and in the Letters of the Poetical Peaſant of Scotland, both of which have been lately preſented to the public by the hand of a writer whoſe

the more ſpeedy modes of ſelf-deſtruction, they in general are driven to the ſlower ſuicide of habitual inebriety. By the more eaſy and opulent, theſe miſerable beings may ſometimes be accuſed of a criminal improvidence with regard to the future; but is it unnatural, or even is it unwiſe, for thoſe not to look forward to the future to whom the future preſents nothing but an unmixed proſpect of daily increaſing miſery and deſperation?

W. W.
Red Lyon Square. J. R.

MEDICAL AND PHYSICAL INTELLIGENCE.

[FOREIGN AND DOMESTIC.]

Report of the Committee on the Vaccine Inoculation at Paris.

SINCE the 3d of Thermidor, the committee have communicated nothing to the public with regard to theſe inoculations. This long interval has not been loſt; the committee are of opinion that they have employed it to advantage.

The firſt experiments, as is generally known, were made with vaccine matter which had been ſent from London. But whether on account of its tedious conveyance hither, or the want of experience in the committee, as yet but little acquainted with this kind of inoculation, this matter, after having been uſed with ſome ſucceſs, at length periſhed in their hands. The arrival of Dr. Woodville, Phyſician to the Hoſpital for Inoculation at London, ſoon enabled the committee to reſume their experiments.

This celebrated inoculator, who was detained at Boulogne by the formalities neceſſary for obtaining his paſſport, had inoculated ſome children in that quarter. This opportunity afforded the committee the means of procuring matter in the ſpace of twenty-four hours, as freſh as it was poſſible to obtain. More children were inoculated in the preſence of Dr. Woodville, and others ſucceſſively ſince that time.

Theſe inoculations, performed with the matter received from Boulogne, have generally manifeſted a more regular progreſs, and more determinate effects, than thoſe which preceded them; and

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whoſe ſplendid and ſolid talents, with equal ſucceſs, have been employed in reſtoring the health of the living, and in embalming the memory of the dead. By ſome, Dr. Currie, in writing the life, may be ſuſpected of having thrown an undue luſtre on the character of the deceaſed Burns; but genius recently departed may be compared to the ſun, which never is obſerved to ſhine ſo beautifully as juſt after it has ſunk below the horizon.

the committee regard the experiments which they have made ſince that time as more to be depended upon. In all the ſubjects of theſe experiments, as well as in the firſt, the diſeaſe has been of the mildeſt kind; not ſo much as one unfavourable accident took place. At preſent the number of inoculations performed under the inſpection of the committee amounts to one hundred and fifty.

The committee have been no leſs induſtriouſly employed in putting under inoculation for the Small-pox, many of the patients who had before been inoculated with the Cow-pox, and whom they conſidered as having been all, more or leſs, infected with it.

Four of theſe children were immediately inoculated on the 3d of Fructidor, three months after the inſertion of the vaccine matter. About the 15th, a ſecond experiment was made upon four of them; and upon ſeven more on the 30th of the ſame month; about two months after their firſt inoculation. Laſtly, on the 11th of Vendemiaire, four other children have been inoculated after the ſame interval.

Of the firſt four children, three have experienced not the ſmalleſt effect from the inoculation. The four who underwent the ſecond trial, have alſo been totally unaffected with it. The ſame has been the caſe with the other ſeven children. On five of thoſe who were laſt inoculated, and on one of the firſt four, ſome effects were obſerved about the place of inſertion; which were, that ſome of them were inflamed, and that there enſued a local tumour, which terminated in ſuppuration. In one of thoſe five children, (named Blondeau, who had been inoculated with vaccine matter, before the arrival of Dr. Woodville), the local tumour was attended with a degree of fever. The others were entirely free from this ſymptom; and upon none of them was there any appearance of general eruption.

In order to aſcertain the nature of the humour produced by this inflammation of the wounded part, the committee took ſome matter from one of the ſubjects of theſe experiments, and made uſe of it for inoculating two children who had never had the Small-pox. The operation was followed by evident marks of variolous infection, ſuch as is obſerved in the common proceſs of inoculation, accompanied with manifeſt fever, and a general eruption. The committee are at this moment repeating the ſame experiment upon the other four children, in whom the inſertion was followed by a ſmall tumour; and it ſhall be renewed every time that the ſame appearance occurs.

Such are the facts which the committee have obſerved, ſince their laſt report to the public and the ſubſcribers. They are far from regarding them as ſufficient to eſtabliſh any decided concluſion on the ſubject. They feel too ſenſibly the importance of the queſtion ſubmitted to their examination, not to conſider it with all the deliberation and all the circumſpection which it requires; and their intention is ſtill to continue their experiments. But there is ground for ſome ſtrong inductions from the facts already collected; and the committee are of opinion that they do not violate the character with which they are inveſted, when they take upon them

to

to make the following obſervations. 1. The vaccine affection appears to them to be of a nature diſtinct from that of every other kind of eruption, and particularly different from the ordinary Small-pox.

2. The vaccine affection appears to them to be of a nature the moſt benign, and which hardly deſerves to be called a malady. Not ſo much as one accident happened to the hundred and fifty ſubjects who have been inoculated.

3. This affection appears to them not to be contagious, either by the air or by contact. Children who kept company with one another during a long ſpace of time were ſucceſſively inoculated, and the infection manifeſted itſelf in none of them before their inoculation.

4. This diſeaſe excites no general eruption. In all the experiments which have been made, no puſtules have appeared, except at the place of inſertion, and even there never more than a ſingle puſtule.

5. The Vaccine Inoculation is no leſs practicable than exempt from accidents, whatever be the age of the perſons on whom it is performed. Infants have been inoculated in the arms of their nurſes; others at the age of one, two, and three years, to the age of fifteen. Perſons of the age of forty, and even fifty years, have alſo been inoculated, and always with the ſame ſucceſs.

6. In fine, the committee have remarked a preſervative effect in the re-inoculations performed for the Small-pox. The nineteen ſubjects ſubmitted to the operation, have been inoculated with freſh pus, taken every time from a variolous infant who was preſent. The committee, for the purpoſe of rendering their experiments more deciſive, employed in many of the ſubjects very deep inciſions, ſuch as, according to the inoculators, neceſſarily occaſion a large eruption of puſtules. They even proceeded ſo far as to introduce, at different times, a great quantity of variolous matter into the inciſions, notwithſtanding which, not one of nineteen who were inoculated, had any thing like a general eruption. In fourteen, the inciſions were ſoon obliterated, without any ſymptom of complaint. In the remaining five, the inflammation can be conſidered in no other light, than as the effect of local irritation, produced by the puncture of the ſkin. The inflammation began the very day of the inſertion. This proceſs has been more rapid, and leſs regular than that of ordinary inoculation. Beſides, inſtances of the ſame effect occur in perſons, who, after having had the Small-pox, have again ſubmitted to inoculation. In a word, if ſome preſervative effect did not operate by the Vaccine Inoculation in thoſe who have ſubmitted to it, how ſhould it happen that the variolous matter introduced into their inciſions for the inoculation of the Small-pox, ſhould have excited (in ſome at leaſt) only a local and partial affection, whilſt, taken from this ſource to be tranſmitted to infants not before inoculated with the Cow-pox, it has produced in the latter all the uſual ſymptoms of general infection?

Theſe preliminary remarks, which, without deciding any thing on

on the fubject, the committee fubmit to the confideration of the learned, coincide entirely with the refult of the experiments made at Geneva, by Dr. Odier, and of which he gives an account, in a report lately publifhed by the prefect of that department. In fix hundred children inoculated with vaccine matter, the mildnefs of the difeafe, its regular and invariable progrefs, its characteriftic of being void of contagion, and the abfence of every complaint fubfequent to inoculation, were conftantly manifefted. One very remarkable circumftance, at the fame time, has afforded an opportunity of evincing its prefervative operation. An epidemic Small pox, of a very malignant kind, having appeared at Geneva, where upwards of a hundred and fifty children were victims to it, and where feventy-fix more have died within the laft month, it was obferved that the children who had been inoculated with the Cow-pox, were totally unaffected by the contagion, except about feven or eight only, who had contracted the infection before their inoculation, and in whom the Small-pox had made their appearance on the fourth or fifth day after the inoculation with the Cow-pox, which in confequence of that accident became ufelefs.

In the Name of the Committee for Vaccine Inoculation,

THOURET, Director of the Medical School.

COW-POX.

The practice of VACCINE INOCULATION, though of recent origin, has already forced itfelf upon the notice of the firft profeffional characters in this and the neighbouring kingdoms. Its advantages are too nnmerous to be detailed here; but it may be proper to obferve, that the vaccine difeafe requires no preparative medicine, no change of regimen, no confinement, and is feldom attended with much indifpofition.

We admit that, comparatively, few have perifhed by Small-pox Inoculation; but it is confiftent with our experience, that numbers have been fo dangeroufly ill, as to excite ferious alarms for the iffue of the complaint. Under the circumftances of Vaccine Inoculation, no fymptom of danger has ever been exhibited. The Small-pox is a difeafe of contagion. The Cow-pox cannot be communicated but by actual contact or incifion; which circumftance enables the inoculator to fingle out any individual for the operation, without fubjecting any other member of the family to the fmalleft rifk.

That the Cow-pox is a fubftitute for the Small-pox is now fufficiently certain. The experiments which have been inftituted to put the queftion beyond all doubt, are numerous, conclufive, and fatisfactory.

Impreffed with a conviction of the truth of thofe facts, and animated by no other motive than a fenfe of duty, and a fincere defire to refcue the lower orders of fociety from the ravages of a contagion, which to them is fo peculiarly diftreffing and fatal, the underfigned Surgeons are defirous to introduce a difeafe infinitely more mild, and very certainly fafe: they are therefore folicitous to recommend to the inhabitants of BRADFORD and its vicinity, the

the practice of Vaccine Inoculation; and they hereby inform the public, that they will attend at their own houſes every Tueſday and Friday morning, to Inoculate, gratis, the children of the poor; and Dr. Moſſman, ſhould any indiſpoſition occur, or other circumſtance requiring extraordinary attention, will give his joint aſſiſtance in carrying every perſon ſo inoculated, ſafely through the diſeaſe.

GEORGE MOSSMAN, M.D.
THOMAS JONES, Surgeon.
W. MAUD, } Surgeons.
THOMAS LISTER, }

Bradford, *Dec.* 4, 1800.

The following ſignatures are added to the teſtimonial in favour of Vaccine Inoculation.

Nathaniel Hulme, M.D.
Gilbert Blane, M.D.
William Blackburne, M.D.
Sir J. M'Namara Hayes, Bart. M.D.
Andrew Thynne, M.D.
Edward Fryer, M.D.
Sayre Walker, M.D.
Michael Underwood, M.D.
Thomas Garnett, M.D.
John Gibſon, M.D.
G.M. Burrowes
David Dundas
Thomas Farquhar
Henry Fearon
James Gilder
John Griffiths
James Higgins
Lewis Leeſe
Edward Leeſe
William Lynn
John Mackinder
Jonas Malden
Joſeph Millington
William Morris
John Pearſon
Thomas Rolph
John Ruſh
Stephen Woolriche

Dr. BRADLEY will commence his Spring Courſe of Lectures on the Theory and Practice of Medicine, on Wedneſday, January 21, 1801, at ſix in the afternoon.

Dr. BATTY will begin his Winter Courſe of Lectures on the Theory and Practice of Midwifery, and on the Diſeaſes of Women and Children, on Monday, January 26, 1801, at half paſt ten o'clock in the morning.—Further particulars may be known by applying to Dr. Batty, at his houſe, No. 6, Great Marlborough Street.

Mr. PEARSON will commence his Spring Courſe of Lectures on the Principles and Practice of Surgery, on Monday, Jan. 26, 1801, at ſeven o'clock in the evening, at his houſe in Golden Square.

Mr. H. LEIGH THOMAS propoſes delivering a Courſe of Lectures on Practical Surgery, in which the different operations will be performed, and the medical treatment fully explained.

The Courſe will commence on Monday, January 26, 1801, at ten minutes after eight o'clock in the evening, at his houſe in Leiceſter Square, where further particulars may be known, or at the Anatomical Theatre in Great Windmill Street.

¶ We

¶ We have inſerted the above at the requeſt of Mr. Thomas, and we conceive, that a courſe of Lectures which will embrace a full conſideration of the medical practice requiſite in Surgery (a moſt eſſential part, and which has not been ſufficiently detailed hitherto), will prove of the utmoſt value and aſſiſtance to the ſtudent. The medical world is ſufficiently acquainted with the anatomical and profeſſional talents of Mr. Thomas, as well as his opportunities for illuſtrating his ſubjects from the collection of preparations of the late celebrated Mr. Cruikſhank, and which he has ſo conſiderably enlarged.

Mr. WILSON will commence his Spring Courſe of Lectures on Anatomy, Phyſiology, Pathology, and Surgery, on Jan. 19, 1801, at the Theatre of Anatomy in Great Windmill Street.

Mr. CHEVALIER's Spring Courſe of Lectures on the Principles and Practice of Surgery, will commence at his houſe, No. 20, in South Audley Street, Groſvenor Square, on Monday, Jan. 26, 1801, at ſeven o'clock in the evening.

Mr. STANCLIFFE, of Cambridge, Lecturer in Chemiſtry to the Middleſex Hoſpital, propoſes to deliver the ſame Courſe to an audience in Birmingham, in the courſe of the enſuing month.

Dr. T. A. MURRAY, of Greville Street, Hatton Garden, will ſhortly publiſh "Remarks on the Situation of the Poor in the Metropolis, as contributing to the progreſs of contagious diſeaſes; with a plan for the eſtabliſhment of houſes of recovery for perſons infected by fever."

We learn that the ſecond edition of Mr. PARKINSON's Chemical Pocket-Book, printed on a fine wove paper, and augmented by the addition of the lateſt diſcoveries, is expected to be publiſhed in the beginning of January.

TO CORRESPONDENTS.

Communications are received from Meſſrs. Cuſtance, Kynion, Jones, Macgarth, Bartley, Spry, Simmons, and Hill; and from Drs. De Carro, Moſſman, and Woodforde, which have been unavoidably poſtponed for want of room.

The great increaſe of original Communications has obliged us to defer an account of ſeveral valuable Books, and the Foreign Literature.

ERRATA.

Vol. IV. p. 514, l. 12, *for* conical, *read* crucial.
l. 15, *for* infraction, *read* impaction.
l. 35, *for* conically, *read* crucially.
p. 515, l. 39, before meaſurement, place the word *ideal.*

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